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SOME REACTIONS TO RECENT CAMBRIDGE PHILOSOPHY (II).

By J. N. FINDLAY.

WE now pass on to show how the theory of language we are considering enables us to deal with the activities of philosophers and with many of the traditional problems of philosophy. Here the illumination derived from the theory is certainly extraordinary: it is as if no one previously had begun to diagnose the philosophical condition or to prescribe its remedy. By comparison no one previously seems to have had an inkling of what philosophers are really doing in their queer, hopeless, passionate disputes, disputes in which there are neither agreed premisses nor rules of argument, and which terminate, with approximately equal frequency, in an impasse, a truism or a paradox. There is all the difference between knowing what philosophers are doing in terms of *such* a theory and knowing what they are doing in terms of their *own* theory, as there would be between knowing what erotic behaviour meant in biological terms and listening to the language of lovers. The effect of the theory is to show us, on the one hand, how confused are many philosophical enquiries, how they spring from a misunderstanding or abuse of linguistic forms, leading to questions which have no answers since they are not properly questions at all, and also to show how there *is* a genuine sense and meaning in some of the most confused of these enquiries. We may also derive from the theory of language in question a new conception of the task of the philosopher which is quite as lofty in its way as the traditional Platonic or Hegelian picture.

Now what have most philosophers supposed themselves to be doing when they faced problems, propounded solutions, or engaged in controversy with each other? They have thought of themselves as men trying to fill in some puzzling gap in their knowledge with unexpected links or connections, or as men trying to explain some unusual effect by postulating the presence of hidden factors or agencies, or as men trying to find proofs for certain plausible but complex theorems, or, alternatively, as hard-headed, sceptical men denying the existence of forces, agencies, media which less rigorous minds had accepted. They have tried to go beyond the veil, to be spectators of all time and existence, to explore the realm of essence, but, whatever they have thought about their problems and questions, they have never thought of them as having a verbal origin; nor have they supposed that, in suggesting solutions for those problems, they were merely suggesting that our usage in regard to certain terms and forms should be altered or supplemented. Verbal difficulties they might admit in plenty, but these were only initial hindrances, to be cleared away before one proceeded with the genuine business of philosophy. Now the philosophy of language we are studying maintains precisely that philosophy has no true resemblance to science or mathematics, that it is not trying to augment our knowledge of reality or to demonstrate new theorems of an abstract, self-evident character, but that its whole endeavour is directed to altering the way in which we *talk* about things. And this endeavour it may carry on confusedly, believing that it is doing something else, and so fail altogether to shed light on the situation, perhaps even making it more obscure and complex. It may, on the other hand, carry on its endeavour consciously and aptly, so as to find linguistic projections which 'do justice', as we say, to unrecognised likenesses and kinships, and give ease to a mind vexed by the 'rigidity', the 'vagueness', the 'inconsistency', the 'confusion', or any other defect of our common diction.

These points will be clearer if we pass from generalities to a consideration of a number of typical philosophical puzzles.

We may first look, for our purpose, at the group of problems clustering around the words 'identity' and 'same', such problems as whether the *same* individual thing could conceivably have had different properties from those it actually has, whether the *same* individual thing can persist through time and have different properties at different times, whether the *same* quality can be present as a characteristic in many different objects, whether different people can see or think of the *same* objects or live through the *same* feelings and experiences, whether the *same* place can be occupied by different objects at different times, and so on. In all these cases there are some people who maintain very passionately that the same thing *can* fulfil the rôles we have just enumerated, whereas there are others who maintain, with equal vehemence, that it cannot be the same, but only a *like* or *similar* thing, which enters into the various situations mentioned. To some it is plain that Socrates sitting and Socrates walking may be 'literally the same' person, whereas others are open to the queer suspicion that the one is *not* the same individual as the other but a *new* individual which has replaced or superseded its predecessor. These suspicions seem particularly acute when it is Socrates' *mind,* rather than Socrates' body, which is under discussion. Again it seems sense to some to say that Socrates might, in some conceivable set of circumstances, have gone to Megara instead of to his death or have married some other woman than Xanthippe, whereas to others it is plain that anyone who differed from the historical Socrates in the tiniest particular, or had any relation which the historical Socrates did not have, would not have been Socrates but a totally different person. In the same way some say that the same universals are 'present' in a variety of contexts, whereas others maintain that the characteristics of particulars are as particular as they are. And while many maintain that we can share a common realm of objects, and fewer that we do or could 'literally' share each other's thoughts, sensations and feelings, others are quite sure that this is utterly impossible.

In all these problems concerning identity it is important that we should not allow ourselves to ask any questions or propound any solutions, before we have asked ourselves how the word 'same' is normally used, and how we ourselves propose to use it in situations where ordinary usage is indefinite. It is important to emphasise that 'identity' means nothing more or less than what we choose it to mean, and that there is no sense of 'same' which corresponds more closely with the 'nature of things' than any other. As regards the identity of concrete individual things, the tests for the use of the word 'same' are also the tests for the use of the proper names of those individual things: if it is correct to use a name N in a given situation and also correct to use it in another situation, we say we are dealing with the same object or entity in both cases. Thus there is a range of observable situations, differing very considerably from each other and involving a complex context of other observable situations, in which we think it correct to use the name 'Stalin': these are the situations we should call 'Stalin sitting', or 'Stalin walking', or 'Stalin signing a death warrant', or 'Stalin entering the Japanese capital', also the very different situations we should call 'Stalin mentioned in a newspaper', or 'Stalin appearing in a photograph', or 'Stalin reported as having occurred in someone's dream', and so on. In all these situations, actual or hypothetical, it would be correct, according to the accepted conventions of our language, to say that we were dealing with one and the same individual, whom we might further describe as a Georgian peasant, the general secretary of the Russian Communist Party, and so on. Now what, for instance, is a man doing who raises doubts as to whether Stalin in various successive historical situations is 'really' one and the same individual, whether he may not in reality be a series of individuals (called 'events' or whatever) which vanish and supersede each other? He is really proposing that we should abandon our very convenient, reasonably unambiguous use of 'same' for a new use according to which a man is only the same entity for a couple of minutes

together, a use which promises no special advantages and would be very hard to carry out in practice. And why is he doing this? In part he is doing this because he is the victim of a confusion. For there genuinely are situations in which we feel tempted to say 'this is the same man I saw yesterday' and yet, on closer examination, we have to admit that this is not a correct statement, but that a new man has replaced the old one. If Stalin genuinely has a series of doubles who replace him on various occasions, then there is sense in wondering whether the man who is bowing to us from the rostrum is the same man who signed the German-Soviet pact. One might find, if one observed more closely or collected further evidence, and made use of accepted tests of identity, that he was not really the same man but only someone closely similar. Now because there are such genuine cases in which it really is not clear whether this, which looks like that, is the same or different, we are forthwith seduced into saying, quite senselessly, that there might be some doubt as to the identity of a person or a thing even in a case where we assume that every test for identity has been or will be satisfied. But though there is all this confusion in our doubt as to the persistent identity of Stalin there is also this amount of illumination in the whole suggested change of usage. Our use of the terms 'same' and 'different' suggests that there is a hard and fast line to be drawn between cases in which it is proper to say 'this is the same as that' and cases in which it is proper to say 'this is different from that'. Now there is no such clear line to be drawn, and there are many cases, actual and hypothetical, in which we simply could not say whether this was or was not the same as that. If a person resembling Stalin externally, and connected with him unbrokenly, sometimes exhibited the traits of a Russian Marxist and sometimes the traits of a quietistic Buddhist philosopher, we might be at a loss to say whether he was or was not the same person. Nor would there be any way of solving our perplexity except by legislating *ad hoc* for this peculiar case. And even where it is clear that some object is the same object we saw some time

ago it is nevertheless valuable to stress its resemblance to cases where, as we say, we have an unbroken series of distinct objects following and replacing each other: the paradox of Heraclitus, that we cannot step into the same river twice, emphasises an analogy that ordinary language ignores, and is accordingly 'profound' and 'wise'. But profundity and wisdom are not a sufficient reason for subverting all our established conventions with regard to the use of the terms 'same' and 'different'. Very similar considerations could be brought up in connection with the problem of accidental properties and 'external relations' referred to above. Would anyone otherwise like Stalin, but lacking one of his qualities or relationships, be the *same* person or someone else? Is it nonsensical to say that Stalin might not have had some property he in fact has, or not have stood in some relation in which he in fact stands? The answer to this is that, as our present usage goes, it *would* be possible for an actual A and a hypothetical B to differ in *some* respects, referred to as 'unimportant' or 'unessential', without forfeiting their right to be called 'identical'. But it is not clear at what point precisely we should give up saying that A and B were the same and prefer to start saying that they were different, or rather it is clear that there is a considerable zone of hesitation within which we should not know what to say. It certainly would not be more convenient to adopt any other usage, but it would at least enable us to satisfy our desire for *situations nettes,* and our dislike of fumbling indecision, as well as our liking for broad and simple rulings, if we laid down that the *smallest* difference in A and B, whether of quality or relation, was sufficient to make them different; this would make everything inwardly unmodifiable and rigidly related to everything else. (We might also have laid it down, if we wished, that any amount of actual or possible difference is compatible with 'fundamental identity', a ruling at the base of the Indian self-philosophy, which affords indefinite linguistic satisfaction to some people.) When we go from these questions to questions as to the identity of the qualities present in various instances,

or of the pains felt by different people, or of the regions pervaded by happenings at various times, we are passing from cases in which there is some degree of uniformity of usage to cases in which everything is wavering and uncertain. Here we are delivered from oppressive perplexity, and acquire a charter of freedom, when we see that we may say precisely what pleases us, provided we realise the advantages and disadvantages, as regards intricacy or simplicity, or illuminating or misleading associations which are connected with any proposed usage.

Having shown how the philosophy of language we are considering enables us to deal with the abstract metaphysical puzzles connected with a logical category like identity, we may next consider its relevance to a totally different group of puzzles, those which concern the reality of an external world, and of the relations of states of consciousness to this world. Since the time of Descartes one of the principal occupations of philosophers has been to try to make us doubt whether material objects really exist, whether we are not dreaming when we seem to see them, whether they do not secretly vanish when we turn our backs on them, and so on; also to arouse less fundamental doubts as to whether material objects are really coloured, odorous, sonorous, extended and so forth. Since the time of Descartes philosophers have also stressed the irrefragable certainty of our knowledge of the 'subject' and its 'acts' and of the perceptual appearances of things to subjects. And they have either tried to prove the reality of an external world, and then tried to connect it with the realm of subjective happenings, or, alternatively, have held that there was no external world, but that tables, chairs, tigers, trees etc. were merely ideas in our own or someone else's minds. Now in order to show how these puzzles arise, and thereby to resolve them, we must consider how such terms as 'matter', 'reality', 'mind' are used in ordinary language or in forms of diction that keep passable terms with ordinary language. We have seen that our language, by its very nature as a language, is concerned with objects such as tigers and trees, that can be

observed by many people in many ways, and that can be successfully shown by one person to another so as to establish conformity of usage. This possibility of observation by many and ostension to many is an actuality in the only sense that has ever been given to it. And the objects revealed in this way are 'physical', 'material' or 'external' in the sense that would be given to these words in any diction not too far removed from ordinary language. Now a very important feature of our statements concerning physical things is that they are all, by their very nature, *corrigible*: if I say 'that is a tiger', 'this is a tree', I can never, by the rules of the game I am playing, incorrigibly establish that what I am saying really is the case, since it is not possible to exhaust the tests to which my statement might be subjected. Thus the mere fact that I *see* a tiger does not complete the 'proof' that there really is one, for I can also hear it or feel it or smell it. Even when I have done this it is still open to me to dissect it, to subject it to chemical tests and so forth. And it plainly would not be a real material tiger if other people could not see, hear, feel or smell it; I am therefore able to confirm its reality over and over again by the simple expedient of trying to show it to someone else. And it plainly would not be a real material tiger if it vanished periodically or became miraculously changed into other forms: every time it does not do this, therefore, it proves its real and material character. And finally it is characteristic of all real tigers that they leave behind them when they perish an endless train of records, effects, memories which would enable anyone sufficiently informed and intelligent to infer that they once existed. If the majority of such records, effects, and memories were mysteriously blotted out, like the records of departing embassies, we should have grave reason to doubt whether we were dealing with a real material tiger. But nevertheless, though the possible proofs of a thing's materiality are inexhaustible, there is a point fixed by good sense and common usage at which it is correct to say that we *know* there is (or was) a tiger present: it is no longer a matter of hypothesis, supposition or surmise, but of knowledge. That

it is logically possible we may be mistaken is nothing to the point: we so use the word 'know' that we know many things concerning which it is logically possible we may be mistaken.

We are now in a position to see where and why philosophical scepticism as to the existence of material objects arises and leads to the 'view' that all our experience is only an orderly dream, and so on. There is undoubtedly a good sense in doubting whether the material objects which seem to be before us really are there and really have their apparent properties: even when we *think* we know they are there and what they are like, we may nevertheless find ourselves completely mistaken. But because all material-object statements are subject to correction, it is easy to slip over into the quite different thesis that even if we were sure that all tests for the reality of a material object were going to be fulfilled, it would *still* be doubtful whether we were not dreaming, subject to an hallucination, deceived by an evil genius, and so forth. It is also possible to hold that, because any finite amount of evidence for a thing's materiality would not preclude the logical possibility of its ultimate refutation, we cannot therefore know that there are material objects, that it is merely an act of faith, a leap in the dark, and what not. Now the former of these positions is essentially absurd since it is only possible to doubt something if one knows what observable situations would make that statement true or false, and also surmises that some of those observable situations will refute the statement. To doubt something without being able to say what situation would resolve one's doubt is merely to assume a pose of doubting without any concrete content whatsoever. But the second position, of maintaining that we do not know that there are tigers, trees etc. because *all* the tests for these things have not been carried out, is only reprehensible inasmuch as it involves an exaggerated and unnatural use of language. For it is good English to say that we know there are trees, that we have *sufficient* evidence to assert that there are such things without a trace of hesitation. On all grounds, then, no sense has been given to scepticism as to

the existence of material objects: there is therefore no reason why we should have recourse to those idealistic paraphrases of our ordinary ways of talking, which think they have explained a great deal when they tell us that trees and tigers are only ideas in someone's mind. Idealism as a theory of the world says nothing that materialism does not also say: only it uses the words 'idea', 'mind' etc. in so eccentrically wide a way as to leave them no clear significance whatsoever. Very similar criticisms would apply to all those theories of material objects which, while admitting their reality, doubt whether they have some or all of the properties they appear to have: colour, sound, taste, smell etc. Now it is clear that, in the only sense ever given to these predicates, it is undoubtedly correct to apply them to material objects. We learn the use of such words as 'red', 'round' etc. by being shown material objects or processes in certain optimal conditions: other people, we note, have been taught to apply the same words in similar circumstances. Now in the only sense ever given to the words 'red', 'round' etc., it is plain that some material objects are red and round, for it is certain that we and others, who regard them in appropriate conditions, uniformly decide to call them so. If there is any *other* sense in which an object may be red (e.g. an incommunicable, private sense, if such were admissible), they may not be red in *that* sense, but they are red in the only sense in which we have been taught to use the word.

It is clear, none the less, that both scepticism and idealism hold considerable paradoxical illumination, if we do not treat them with the wrong seriousness. For it is worth while saying that there is always a logical possibility of error in our material-object statements, and it is worth while stressing that the line between the real and the phantasmal is not an absolutely clear one, and might, in perfectly conceivable circumstances, become impossible to fix. If an object could be shown to A, B and C, but not to D, E and F, or if it vanished and recurred at intervals, we could not say, with our present rules of language, whether it was real or not. And it may be worth while, from an ethical point of view, to cultivate

towards material objects, at least at times, that attitude of slight consideration which we normally adopt towards objects we regard as imaginary.

It would be interesting to pursue our linguistic analysis a little further, and show how philosophers have arrived at the view that we have an immediate, incorrigible awareness of our own mental acts of the moment, that these mental acts take place in our own bosoms and cannot be shown to anyone, that the behaviour in which they issue is merely an outward and visible sign of them, that we can never certainly *know* that such mental acts take place in others, and so on. All these 'views' have a foundation in our language, they spring from an exaggeration of certain significant ways of talking, but they all lead, as they stand, to 'posers' and mysteries which render them more confusing than helpful. If we considered carefully what we can and what we cannot say, by the very nature of our language as a language, we should be able to resolve the majority of these puzzles, retaining whatever modicum of picturesque illumination there is to be found in them. We might also with profit pursue our philosophy of language through other puzzles which arise in different fields: the problems of universals, of relations, of various logical and mathematical categories, of causation, of induction, of mechanical and biological categories, of space and of time, and so on. Until one sees the variety of its applications, one can form no notion of its fruitfulness. Enough has, however, been said to make plain what the essential principles of our philosophy are, and how they work out in practice.

Enough has also been said to make plain what we previously said of the view taken by our theory of the essential nature of philosophical activity and of the proper task of the philosopher. If the philosophic impulse is, on the current confused view, merely a higher flight of the scientific impulse, it is, on the view we are expounding, an impulse different in kind from the scientific impulse, a fact genuine philosophers have always obscurely recognised. For it does not spring from any dissatisfaction with what we know, but, what is far

more weighty and deep-seated, with the way in which we say it. No philosophy augments our knowledge of reality by one iota, but it can alter our way of saying things in a highly misleading or a highly illuminating manner. Philosophy arises, in part, out of our confused wonder at the queer suggestions of linguistic expressions whose use has been forgotten. We wonder what adjectives 'stand for', or what numerical expressions refer to, and are led to affirm the existence of some very queer entities with extraordinary properties. We are baffled by the meaning of mental terms, and wonder how in the world a man can think or believe what is not the case at all, and construct some theory to meet it. We may fail to see how mental language is connected with physical language, and so be forced to invent the various 'theories' of the mind-body relation. In all these cases philosophical construction rests mainly on a confusion; the task of a genuine philosophy is to trace such confusions to their roots, to show why we are tempted to say what we say, and to indicate how little illumination or explanation we can derive from it. But there are other cases where philosophical construction springs from the fact that our language recognises only a few of the multitudinous analogies among the things of our experience, and that other analogies press in upon us at times and create a stress which cannot be relieved till we have given them *some* recognition in our language. We are then tempted to abuse ordinary language, to utter paradoxes of various kinds, in short to create new usages which may ease or vary our handling of the world. To say we do not know whether our desk is real or whether our wife suffers pain is to abuse language, but also to stress an analogy, for there are deep resemblances between waking and dreaming or between cunning automata and intelligent persons: there is also no clear dividing line between surmise and belief, or belief and knowledge. Again to say that value-statements are not really statements but interjections, is to abuse language, but it also brings out an unrecognised analogy and stresses an unrecognised difference. And who can deny

that the river of Heraclitus into which, very oddly, we cannot step twice, or the charmed arrow of Zeno, at rest throughout the course of its flight, has brought out features of change and process which our ordinary language fails to emphasise? The metaphysicians of the past often wrongly supposed that they were building up ontologies: they were in reality, in many cases, doing something far finer, creating new languages which bring out certain analogies more pointedly and more systematically than is possible in our current language. In any case there is nothing in the philosophy of language we are studying which renders it hostile to any form of philosophy, however great the element of confusion these may hold. It has little affinity with those trends of thought called 'positivistic' whose aim is to eliminate philosophical perplexity in order to 'get on with the work of science'. There may, from its point of view, be far more importance in the confusions of a Locke or a Kant, far more illumination in the exaggerations of a Berkeley or a Spinoza, than is to be found in the clearest papers of the best experimentalists. The effect of this philosophy is not to diminish our faith in the value of philosophy but rather to augment it: for it as for Plato *καλὸν τὸ ἆθλον καὶ ἡ ἐλπὶς μεγάλη.*

THE MEANING OF "EVOLUTION".

By Q. B. Gibson.

It is a commonplace that the steady introduction of the "idea of evolution" was one of the main changes occurring in scientific thought in the nineteenth century. There arose theories of the evolution of the earth's surface, of the evolution of species, of the evolution of human society. Together with these, there arose speculative philosophical theories which made assertions about the evolution of the world as a whole. "Evolution" has thus come to play an important part in both scientific and philosophic thought.

But there nevertheless remains a dangerous obscurity about the use of the word. Are the scientists and philosophers who use it referring to a special sort of change or not? And if they are referring to a special sort of change, what is it that distinguishes it from other sorts, actual or possible? For the scientists these questions are perhaps not important, and may not affect their practical conclusions. But when philosophers speak of an evolving universe, they should be required to make clear what evolving is.

Consider the first question—Is evolution meant to be a special sort of change? When we speak of looking on something in terms of evolution, or from an evolutionary standpoint, it might be thought that we are referring, not to a special sort of change, but rather to any sort of change taken in a certain context. Such a context occurs when, in speaking of anything temporal, we distinguish between examining its constitution as we find it at the present moment and examining the changes which have occurred to it in the past and have led up to its present state. When we examine it in the latter way in contrast to the former, we may be

said to be considering it in terms of evolution. To take up an evolutionary standpoint with regard to anything would thus be simply to look at its history. If this is all that is involved in the use of the word "evolution", to speak of an evolving universe would be the same as speaking of a changing universe, with the difference that you would be exhibiting a special interest in its changes.

But is this all that is involved? It is certainly true that when we look at something in terms of evolution, we are looking at its history. But are we not also supposing that there is something peculiar about its past changes which makes it important to look at its history? I would never speak of looking on my watch from an evolutionary standpoint. This is not just because its history is dull. The dullness of its history is due to something in the nature of the changes which it undergoes, and it is this something which seems to prevent us from calling its history an evolution.

To verify this, we must tentatively broach the second question, and search for some distinguishing mark which makes evolution a special sort of change. If no such mark is to be found, we will have to conclude that the use of the word "evolution" has no special significance. So far we have merely been led to suspect that some such mark can be found.

The search for such a mark is just what seems to be missing in the two interesting articles on evolution by Mr. Partridge in earlier issues of this Journal. The value of Mr. Partridge's treatment is that it warns us against two alleged distinguishing marks of evolutionary change—firstly the presence of some single force or power which necessitates such change, and secondly the requirement that the change be progressive. We must straight away agree that when we ordinarily speak of evolution, as in "the evolution of species" and so on, neither of these characteristics is being asserted of the change. We can agree about this without committing ourselves to saying that no such evolutionary force exists or that what we call evolution is not on the whole progressive. The questions of the progressiveness of evolution and of the

presence of a single force which determines it may be left to be judged on independent evidence. What concerns us here is that "evolution" as we ordinarily use it, is firstly not a proper name standing for some unique particular entity but a common name standing for certain characteristics possessed by certain actual or possible changes, and secondly that it is not to be identified with progress. Anyone who chooses to use the word in either of these ways may of course do so. But I am accepting at once that we do not ordinarily do so, and am therefore intending to continue the search for the distinguishing characteristics which lie behind the ordinary usage.

It is here that Mr. Partridge does not follow. His main concern is to show that the existence of evolutionary change creates no new problems for logic. If logic is (to use Mr. Partridge's words) "the study of things as they occur in propositions", this certainly seems to be true. Propositions about evolutionary changes and about ones that are not evolutionary may not show any difference *as propositions,* i.e. in their form. But this should not lead us to neglect the search for any difference between evolutionary and other sorts of change. To say that the two are logically the same is only a negative account of the matter. May there not be a distinction here which is of importance for metaphysics, if not for logic?

The simplest distinction which comes to mind is that between repetitive and non-repetitive change. We do not speak of evolution where the change brings about a repetition of what has gone before. We would put it another way by saying that history only becomes evolution when at each stage something *new* comes into existence. Absence of repetition, or novelty, we might say, is the criterion of evolution. This will fit in very well with the suggestion that it is those types of history in which we are interested which we call evolution. For it is just where there is repetition that history becomes dull.

This suggested criterion is not valueless—it is merely extremely vague. If we are looking for the distinguishing characteristic of evolution, we must also consider what is meant by there being absence of repetition, or novelty. That the three notions are somehow bound up together is an important fact. What we want is to clarify the meaning of all three of them. To do this it is necessary to consider carefully some of the possible sorts of change. We can then note various shades of meaning of novelty and non-repetition, and decide which of these, if any, can be made the characteristic of evolution.

The first distinction to notice between different sorts of change is the common-sense one between the change of state of something which endures through the change and the change which occurs when something becomes something else. The familiar trouble with this distinction is that the line of division is arbitrary. What sort of change is it when an axe first gets a new blade and then a new handle? In the face of such problems, we seem forced to one or other of two extremes. The older way is to say that the ultimate things in the world never cease to be or become something else, but rather that all change is change in the state of these things. The particles of which the original axe was composed, it is said, have not ceased to be, even though none of them remain in the reconditioned axe; and similarly the particles of this axe after reconditioning have not just now come into existence but have existed eternally elsewhere. We are here in a world of indestructible atoms and eternal souls. The other extreme is to say that there are no enduring things at all, but that all change is transition from event to event, or occasion to occasion. The axe on this view is nothing but an historic route of such occasions, each of which is at every instant becoming something else. We are here in a world of process of the sort conceived by A. N. Whitehead.

Now before passing on, we should consider whether we really need to go any further. It might be said that we have

already found the criterion of evolution, that evolution is just the change of something into something else, and that this is just the sort of "novelty" we are looking for. If this were so, of course, we could not continue to hold that some changes in the world were evolutionary and some not. As we have just seen, we would be driven either to the view of a world of indestructible atoms in which evolution would be impossible, or to the view of a world of transitive occasions in which every change would be evolutionary. But it might be said that this is just the case, and that to maintain the existence of evolutionary change, we must abandon the world of indestructible atoms, and accept the transitive occasions.

Is this so? Would evolution be impossible in the former world, and would all change be evolution in the latter? No; I think not. It is not because of the indestructible particles so much as because of the *sort* of change which it is alleged occurs to these particles, that the former world might be thought of as barring evolution. And it is not because of the transition from occasion to occasion so much as because of the *sort* of change which it is alleged takes place in this transition, that the latter world might be thought of as containing nothing but evolutionary change. The distinguishing mark of evolution, that is to say, may lie among the further characteristics of changes which are generally accepted by upholders of the "occasion" view and rejected by the holders of the "particle" view. If on examining these further characteristics we get what we want, we may be able to abandon this first suggested criterion of "change of something into something else".

There is one such further characteristic which seems to suit us admirably. What is required for evolution, we might say, is that the change be *qualitative,* not quantitative. It is not perhaps the belief in eternal particles which would preclude the possibility of evolution, but rather the view that such particles only undergo quantitative and never qualitative change. And on the other side it is not perhaps the belief that the ultimate entities are occasions which would

make all change evolutionary, but rather the view that the change involved brings into being something qualitatively different.

This distinction between quantitative and qualitative change, however, is itself obscure and requires further examination.

It is not being very deep to say that any entity, whether enduring or not, must possess qualities. It is easy to see then that change may occur through the altering of these qualities. One quality may cease to be and another take its place, or a quality may be simply gained or lost. The difficulty then is not so much with qualitative change as with being able to point out precisely what sort of change it is that is not qualitative.

There are at any rate two types of change which can be classed as quantitative. The first is *change in intensity* of qualities. Given that a thing has certain qualities, we can conceive of these becoming more or less intense without giving way to different ones. The second is *change of position, or motion.* We find this, of course, only in the case of entities which are spatial. Every spatial entity not only possesses certain qualities, but also occupies a certain position. It may be said that occupancy of a certain position is itself a quality. If so, motion would have strictly to be classed as a type of qualitative change. But at the very least we can say that occupancy of a position in space is a very special sort of quality, and for this reason it is still important to distinguish motion from change of quality in the more ordinary sense.

These two types of quantitative change, as far as I can see, cover all cases. They differ very greatly from each other, the essential point that they have in common being that in both cases there is the possibility of dividing the change up into unit changes, and saying that the full change is equivalent to so many unit changes. This is impossible in the case of ordinary qualitative changes. Take, for example, continuous change of colour from green to blue. We cannot take a unit of change near the beginning, and say that the full change is equivalent to a number of these unit changes. There is no

sense in speaking of any "amount" of change near the blue end which is the "same amount" as that of any unit near the green end. But the amount of change in position can be spoken of in terms of units. And the amount of change of intensity can be spoken of in terms of unit changes in degree of intensity. It is in virtue of this that these changes can be measured, while qualitative changes cannot.

In the selection of units for the measurement of changes, we should notice that change in position has an advantage over change in intensity. There is a great variety in the types of quality which may vary in intensity, and each type requires its independent unit. The change in intensity of a given sound, for example, cannot be measured in terms of a unit change in intensity of light. But all changes in position can be measured in terms of unit change of position. I am not only thinking of the case of motion in a straight line. You can divide up changes in position in terms of unit changes in direction as well as unit changes in linear motion, and in this way all such changes without exception can be compared "quantitatively" with one another. This is probably the main factor in making motion much the more important type of quantitative change.

Let us now consider this case of motion a step further. So far we have been simplifying the position by considering change occurring to only one thing (or, if you like, through one single strand of successive events—for convenience I will relapse into the assumption of enduring entities from now on). This, however, will not do. On one view concerning space it would not even make sense to speak of a thing changing position, except in relation to some other thing. And it is certainly true that we cannot measure any change in position except by referring to other things. Let us consider what happens then when a number of things are changing position at the same time.

We realise at once that these things form a *pattern*. Taking a group of things together, each of the things has a given spatial relationship to every other. If one or more of

the things move relatively to the others, the pattern is changed. Change of pattern, or shape, cannot be broken up into unit changes, but must be classed as a qualitative change. We thus have the interesting position that while individual things are undergoing the quantitative change of motion, any group in which these things are included must be undergoing qualitative change of pattern. We cannot have the mere quantitative change without having at any rate one sort of qualitative change.

If the things in a group were to undergo precisely similar changes in position, it might of course still be said that there was no qualitative change taking place. But even in this case, as soon as you include in the picture a system of reference in relation to which the group of things is moving, the qualitative change of pattern is at once re-introduced.

These remarks may throw some light on the difficult notion of "complexity". Complexity seems to have to do with the arrangement of parts in a whole—to a large extent, if not entirely, with the arrangement of spatial parts in a whole. The complexity of a whole does not seem to be affected by qualitative changes taking place in the parts, nor by any changes in the intensity of any of the qualities of the parts, nor even by change in the number of the parts. Change of complexity then seems to be nothing but a special type of qualitative change of pattern. Just how it differs from other sorts of change of pattern, it would be difficult to say. Perhaps it would be a help to say that one whole is more complex than another when there is a greater diversity in the types of relationship between the parts. For example, a three-dimensional pattern has a greater diversity of relationships between its parts than has a two-dimensional pattern, and that is perhaps why it is called more complex. However that may be, what is of interest at the moment is that, as a qualitative change of pattern, change of complexity (I am coping here only with complexity of spatial wholes) only comes with the movement of the parts which form the complex

whole. Growth in complexity, though a form of qualitative change, is always directly dependent on quantitative changes.

This connection between motion and change of pattern takes us over from considering quantitative changes to considering qualitative ones. Imagine a world in which all changes were quantitative changes. This would consist of particles (or strands of occasions) which always remained qualitatively the same but which were always undergoing redistribution among themselves. Such a world, we have now seen, must contain at least one type of qualitative change—change of pattern. This type is completely dependent on the presence of quantitative changes If I am right in saying that changes of complexity are essentially a variety of change of pattern, they must, where they exist, be likewise dependent on quantitative changes.

But the world as we know it contains also many other types of qualitative changes, from changes in sound or colour to the subtle changes of human consciousness. What are we to say of the relation of these to quantitative changes? Here two alternatives present themselves. We might say that they, like changes of pattern, are really dependent on the movement of particles. Or we might question this and admit the existence of independent qualitative changes. On the former view, all scientific laws could be stated in terms of the regularity in the movements of qualitatively unchanging particles. On the latter, we could admit independent laws of regular sequence of qualitative change, quite irrespective of any alleged relation with quantitative changes. Perhaps there are no such independent qualitative changes, but it would be a mistake to ignore the possibility.

Among these qualitative changes, whether independent of quantitative ones or not, there is a further very important distinction which we can make. When any quality appears, it may already have appeared previously in the course of time, or it may not. If we restrict ourselves to completely determinate qualities, we can see at once that such qualities are constantly coming into existence for the first time. In any

reasonably complex whole, for example, any redistribution of parts is almost certain to produce a pattern which has not appeared before. Even among such simple qualities as shades of colour, it is always quite likely that some determinate shade may appear for the first time. On the other hand it is possible for a qualitative change to give rise to a quality which has been previously manifested. In other words, it might reproduce a state of affairs exactly similar to a previous one.

This distinction is undoubtedly of importance, but it needs to be supplemented by another one. Among those changes which give rise to qualities that are dissimilar from previous ones, we might make a gradation according to the *degree* of dissimilarity produced. Some qualities will appear which will be very like old ones, and some which will be very unlike. Stated another way, there will be differences in the height of the lowest common determinable under which the earlier and the later determinate qualities fall. A pattern that appears for the first time, for example, may be very like previous patterns. But if a shade of colour were to appear in a world where there has been no colour before, we would have a quality which was very unlike previous ones. We might reach a point in fact where we could find no common determinable except the most general determinable "quality". This would seem to be the case with a change which gave rise for the first time to a colour, or to a form of life, or a form of consciousness.

This examination of qualitative and quantitative change has been lengthy and not very systematic, but it has been designed with a purpose—that of clarifying the meaning of "evolution" and the allied notions of "novelty" and "absence of repetition". Let us turn our attention back to these.

It is now clear that there are various shades of meaning when we speak of novelty:

(1) There is a general sense in which novelty is inseparably bound up with the occurrence of events in time.

No particular event can ever have happened before. Time itself debars this sort of repetition.

(2) We might restrict novelty to the coming into being of new *things*. In this sense a world of indestructible atoms would preclude novelty, whereas in a world of occasions there would be origination of novelty with every instant. Repetition would here mean the recurrence of identical things throughout their changing states.

(3) We might restrict novelty to the appearance of qualitative changes. In this sense neither change in position nor change in intensity would be regarded as in themselves bringing anything new. Whatever qualities things had would be "repeated" in different positions or with different intensities. Qualitative change of pattern must of course always accompany relative changes of position; so that even in a world of qualitatively unchanging but moving particles, there would always be some novelty in this sense—viz. "novelty" of pattern.

(4) A further restriction which would avoid this would be to say that novelty only comes with *independent* qualitative changes. This would exclude not only qualitative change of pattern, but also any qualitative change which was found to be dependent on quantitative change. This is the sense in which we use "novelty" when we say, for example, that there is no genuine novelty about the change in the pitch of a sound, because it is "really" just an increase in the amplitude of vibrations in the air.

(5) Or alternatively, we might say that it is not so much the independence of qualitative changes which is required, as that they must give rise to qualities which have not been manifested before. This is a very usual sense of the word "novelty". It is what we mean when we talk about new qualities appearing. I will simply refer to it from now on as "qualitative novelty". It is where you get exactly similar states of affairs recurring, that you most naturally speak of repetition.

(6) A final restriction on the meaning of "novelty" comes when we require that the dissimilarity of the present quality from previous ones must be high. We might eliminate as not really new, those qualities which approximate to old ones. We might even say that genuine novelty only comes with those changes in which the quality which is coming into existence has no common determinable with any previous one, save the most general determinable of "quality".

So much for "novelty". It is now quite clear that it is not of much value as a criterion for evolution. If we are to attach any value to the term "evolution" as indicating a special sort of change, we must ask what sort of novelty, and what sort of absence of repetition it demands.

We can straight away eliminate senses (1) and (2). If evolution only demanded novelty in sense (1), it would stand for any sort of change. And we agreed that we should only retain the criterion of novelty in sense (2) if we could find no further characteristic, generally associated with the coming into being of new things, which would fit better. Such a further characteristic we seemed to find in change of quality, and so we pass to sense (3). It certainly does seem clear that motion from one position to another can never constitute evolution, nor can mere growth in intensity of qualities. That is why evolution does seem to be somehow bound up with qualitative change.

It would be rash, however, to assume that any qualitative change constitutes evolution. Some further restriction may be required. Must we, for example, demand that the qualitative changes be independent of the movements of unchanging particles? Or again, must we require that any quality or complex of qualities which comes into existence be "new" in the sense that nothing exactly similar to it has appeared before? We are still asking for the verdict of ordinary usage.

Such a verdict would, I think, come down on the side of restriction of evolution to the appearance of the "qualitatively new". Independence of quantitative changes, where required at all, seems to play a subsidiary part.

But the appearance of such new qualities covers, as we have seen, a very large field. New determinate qualities and complexes of qualities are every moment coming into existence. Even a redistribution of parts in a whole means change to a pattern which is extremely unlikely to have appeared before, and such likelihood is even less in a whole world of moving particles. Such a world would therefore have to be classed as an evolving one.

This seems unnatural, and we must consider why. It seems to be that the "extreme unlikelihood" of the previous appearance of the pattern is not enough. If in any series of changes it is theoretically possible at any point that we are returning to a state of affairs exactly similar to a previous one, if, in easier language, it is theoretically possible that the series of changes is cyclical, then we may say that evolution has not taken place. If the movement of particles is subject to laws, and if we were omniscient, we could predict all future redistributions, and could then read off the answer to the question whether the world process was cyclical or not. But without such omniscience, there is nothing in the quantitative laws of motion to tell us that exactly similar qualitative patterns will not recur. We therefore do not speak of evolution.

There is, however, one sort of qualitative change of pattern which is often spoken of as evolution, and this helps to illustrate our point. This is the change from the simple to the complex. Just like any other change of pattern, change in complexity appears as the direct outcome of the movement of parts. The difference is that a complex pattern cannot possibly be exactly similar to a simple pattern, so that change from one to the other cannot possibly be cyclical. There may of course be a change from simple to complex and then back to simple. In this case the *whole* process could not be called evolutionary, because there is the reopened possibility of exact recurrence. But if the movements of parts were found regularly to bring about changes from simple to complex (or vice versa), we could say the *whole* process was evolutionary,

and we could formulate a law of evolution, which would be a qualitative law concerning changes of pattern.

We may apply this principle not only to "qualitatively new" patterns but to *any* determinate quality which has not appeared before. Take the case of the evolution of living organisms. No such organism is exactly similar to its parents, just as no pattern of parts is exactly similar to that existing before the movement of the parts. With every organism we have certain qualities which have not appeared before, however slight the variation may be. But such changes in themselves will not constitute an evolution of organisms, because we have no guarantee that there will not be a recurrence in the history of any organism's descendants of qualities which are exactly similar to those of the organism. Whatever the laws of such variations are (whether they are independent qualitative laws or are ultimately dependent on movements of particles does not matter here), there will be nothing in them to preclude cyclical recurrence. It is only where we get variations in qualities which we *know* cannot have occurred before, such as we get with new *species* of organism, that we speak of evolution. If organisms of a new species were to appear and then die out, this process would not be evolutionary because of the reopened possibility of the exact recurrence of qualities. But if the variations in organisms were found regularly to bring about changes to species, certain of which did not die out, then the whole process would be evolutionary.

From the point of view of the appearance of new qualities, these two examples I have given seem to be precisely parallel. And yet some people would say that the changes in species of living organisms constituted evolution, while mere change in complexity of pattern did not. How are we to explain this? It is here, I think, that we must introduce the distinction between dependent and independent qualitative changes. It is just those people who take variations in organisms to be *independent* of changes in position of qualitatively unchanging particles who say that such changes

constitute genuine evolution while changes in complexity do not. It is agreed by all that changes of complexity are not independent in this way. If you say that changes in the qualities of organisms are likewise not independent, you are unable to distinguish them from the others as genuinely evolutionary. This, I take it, is just the position of the "mechanist" in biology. But if you are not a mechanist in biology, and admit changes in biological qualities conform only to independent qualitative laws, you may try to back yourself up by saying that you are allowing for genuine evolution while the mechanist is not.

This of course would not be substituting the criterion of independence of quantitative change for that of the appearance of new qualities. There might well be independent qualitative changes, subject to independent qualitative laws, which would lead to the exact recurrence of qualities, and I doubt whether these would be regarded by anyone as constituting evolution. What is here demanded is rather a *double* criterion. First of all you must have the appearance of qualities which have not appeared before, and then secondly you must have the appearance of these qualities independently of quantitative changes of particles. I doubt whether this double criterion is essential to the ordinary usage.

One point remains. I have still not considered the question of the extent of the dissimilarity of the new qualities from the old. I have dealt only with differences of determinate qualities, and these may be very slight or very large. We can now go a step further and point out that the greater the dissimilarity, the less likelihood do we consider there to be of exact recurrence, and hence the more likely are we to speak of the change as evolution. The difference between organic species appears greater than that between simple and complex patterns, and this may be one factor leading some to speak of evolution in the former case and not in the latter. But when we come to the dissimilarity between the qualities of a complex of particles and the qualities of a living organism (or, to take another example, between the qualities of a living organism

and the qualities of consciousness), we see that this is much greater still. The change which occurs, then, when any quality of an organism or of a consciousness appears for the first time would be the most certain of all examples of evolutionary change. Once such a radically different quality appeared, you would have the very best guarantee against cyclical recurrence.

We are here face to face with the notion of "emergence". This term tends to be reserved for just those appearances of qualities which are radically dissimilar from any which have appeared before. As I have stated it, this case of emergent qualities is nothing more than an extreme case of something which is found at every moment in the world's history. Why is it then that some people regard "emergent evolution" as a special sort of evolution which it is very important to draw attention to, while others see some peculiar difficulty or inconsistency in the conception?

The answer seems to be concerned with the possibility of *prediction* in accordance with laws. Once we have formulated laws concerning the movements of qualitatively unchanging things, it is possible for us (theoretically) to predict future movements and positions with complete precision. This means also that we can predict future patterns with complete precision. Evolution from simplicity to complexity is thus in principle compatible with complete prediction. When we come to qualitative changes of which we are unable to see the dependence on underlying quantitative changes, we lose this capacity for complete prediction of new qualities. When a new determinate quality appears, we could have told from qualitative laws what *sort* of quality it was going to be, but not what precise determinate quality. We would still possess some capacity for prediction, but not for complete prediction. The less similar the new quality is to the old ones, the more general must be the terms in which it could have been predicted. In the face of very large variations appearing in organisms, for example, we might be forced up to some such generalised prediction as this—that any future

variation must at least possess certain general organic qualities.

But when finally we come to a change from organic qualities to a newly appearing conscious quality, we seem to have got to something which could not have been predicted even in the most generalised terms. This is because the conscious quality comes under no determinable kind of quality which has appeared before. Hence such changes have to be barely accepted—with or without "natural piety".

The conclusion here then seems to be that there is no incompatibility between most evolutionary changes and the possibility of their being predicted. They can however be ranged in a scale as more or less precisely predictable, and at the top of this scale we may find changes which are completely unpredictable. This does certainly give a special status to "emergence", but "emergence" nevertheless remains a limiting case of evolutionary changes in general, and should therefore be regarded neither with special abhorrence nor with special reverence.

This discussion of evolution has been concerned with the use of a word. It has not been its purpose, however, to say how the word ought to be used. I have not even been concerned with the precise way in which different writers have in fact used it. I have merely tried to state in unambiguous terms the sorts of changes to which in its ordinary usage it seems to refer. So long as this distinguishing of the sorts of change is of value as a basis for the further factual questions as to what changes actually do occur in the world, and in what relationship to one another, it is of no real concern whether the particular word "evolution" is used or not.

THE MORAL PHILOSOPHY OF HOBBES.

By J. A. Passmore.

The more unorthodox a philosopher is, the more subject he will be to misinterpretation. There is a constant tendency to assimilate a philosopher to some general type, to see in him what is most familiar and what we feel most at ease in refuting. It is especially simple to do this, in that the unorthodox are usually inconsistent; they become frightened at their own presumption or, perhaps, fail to see themselves how far-reaching are their innovations. Thus while Hume's innovations in philosophy have no necessary connexion with the theory of ideas and present problems even to those who reject that theory, it was not at all implausible to treat him as a Lockian in the manner of Green, and by this means to "refute" him, without ever coming to terms with what is novel in his philosophy. Similarly, Hobbes has been regarded primarily as an exponent of "the social contract"; a conception which, whatever the emphasis laid upon it by Hobbes himself, could quite well be omitted from his general theory. If, indeed, we take as central "the social contract", then we would be obliged to argue with Laird that Hobbes was not in ethics an innovator, that his doctrine and technique were alike mediaeval—and we are then quite incapable of accounting for the stir which the "Leviathan" undoubtedly created. An alternative approach, however, has been indicated by Strauss;[1] and, even if we are in the end unable to accept the Hobbes he presents as the one who is of real importance to moral and political theory, he has shown how the problem of interpreting Hobbes has to be approached, by

[1] In his "Hobbes' Political Philosophy" (Oxford, 1936), to which this account is heavily indebted.

demonstrating that we are not to take the inconsistencies so notable in Hobbes as mere accidents, but as displaying the conflict in his work between two distinct tendencies, which one may call the naturalistic and the rationalistic.

It is necessary to remember, in the first place, the conditions under which Hobbes was led to put forward his theory of morality, and to place such emphasis upon the sovereign power. It is not an accident that we find such an outburst of moral theorising in the Britain of the seventeenth and eighteenth century. Such a phenomenon is inevitable when the accepted criteria of moral conduct are no longer acceptable, and the more so when even the code itself has become a subject of dispute. The Renaissance drew attention to a morality different in many respects from that of Christianity, and yet not obviously inferior to it; the development of capitalism was making it clear that many of the precepts which had been widely regarded as "laws of nature" (e.g. that no interest on loans should be taken) were not after all consequences deducible from the nature of things, but were dependent upon the social structure of mediaeval society. In this way, the stage was set for a revival of the Sophistic historicism in ethics, which could only be combated by a new demonstration that morality was "eternal and immutable". The effect of the Reformation was to make such a demonstration an even more urgent task. It had been sufficient to make morality dependent upon theology, at a time when there was in existence an institution which could mediate between God and man; without such temporal intervention there was no way of discovering the divine will. The object of the Reformation, it is true, was to substitute Scripture for Church, but it soon became evident that reliance upon private judgment gave birth to an endless variety of sects, differing not only in theology but in morality and politics. It is not surprising that there was in the seventeenth century a widespread fear that the evident breakdown of mediaevalism would precipitate anarchy. It was clear that the old order in politics, in religion, in human culture generally

was breaking down; it was not at all clear that any new order was going to take its place.

Hobbes was particularly sensitive to the intellectual and social disturbances of his own time; and his reaction to them was to set up a theory which would establish the conditions under which social order was possible. This was not merely an intellectual exercise; he hoped to persuade men that if they persisted in their present courses, anarchy was the only possible outcome. That "war of all against all", which he painted in such lurid colours, was not an event in the past, from which we had now safely escaped; it was something to be feared whenever men forget the conditions under which alone civil society can survive. He is not to be thought of, then, as a destructive critic of conventional morality, concerned only to undermine our ordinary beliefs, and to substitute for them blind obedience to the ruling powers. On the contrary, his object is rather to support conventional morality by showing it to be bound up with social order; but equally to contend that there can only be social order where there is obedience to the constituted sovereign.

It is in the "De Corpore", where Hobbes is considering the utility of philosophy (Part I, Ch. I, § 7), that he gives the clearest, although by no means the only, statement of his general purpose. The special utility of moral and civil philosophy, he argues, is that they help us to avoid calamities, of which the chief is war. War is not something which men desire; it arises because they are ignorant how to avoid it, because "there are but few in the world that have learned those duties which keep men in peace, that is to say, that have learned the rules of civil life sufficiently". The knowledge of these rules is "moral philosophy". It is true, Hobbes says, that he has had many predecessors who have written of morals, but these have been rhetoricians rather than scientists. "What is chiefly wanting in them is a true and certain rule of our actions, by which we might know whether that we undertake be just or unjust. For it is to no purpose to be bidden in everything to do right, before there be a certain

measure of right established, which no man hitherto hath established." Hobbes had as his object to find such a rule.

That is why Hobbes was a secularist in morals. The Churches could not speak with a united voice, so that if we were merely instructed to pay heed to them, there would be no general rule established in a community. This means that there could be no justice, because to act justly towards our fellow-men is to give them what is due to them, and if their conception of what is due is different from our conception of what is due, there is no way of telling whether our action is just or unjust. It is only when we have regularised relations with our fellow-men, only when what is due is established by some law which we both recognise, that there can be any such thing as justice. And if the Churches try to enforce a moral code different from that established by law, their pretensions must be opposed, if any kind of civil society is to be preserved. For all that, Hobbes was influenced by the theologians more than he realised. He took it for granted that only a precept of some kind could supply a common rule of good and evil; it was left for Cudworth, under the influence of Plato, to attack the whole conception of a preceptive ethics.

"Whatsoever is the object of any man's Appetite or Desire", Hobbes argues, "that it is which be for his part calleth Good; and the object of his Hate and Aversion, Evil. . . . For these words of Good and Evil are ever used with relation to the person using them; there being nothing simply and absolutely so; nor any common rule of Good and Evil, to be taken from the nature of the objects themselves" ("Leviathan", Pt. I, Ch. VI). Thus if all we had to rely upon were the judgments of individuals as such, there would be no common rule; men make various demands, and regard as good whatever satisfies these demands, and there is nothing which all these objects of demand have in common. Certain activities and certain ends may be generally valued because a great many people want them (just as economic goods have a price); but

this does not constitute an obligation upon us to demand them, and such a value would fluctuate with the whims of individuals. Now, it is not possible to overcome this difficulty by setting up some wise authority who will decide what is really good; as Hobbes points out in the "De Cive", if we knew who the wise authorities were, we should already know what is really good. Yet we must have some regular standard, and this standard can only be the decision of *some* person, since there is no other way of establishing what is good. Consequently, we have no alternative but to accept the decision of an arbitrator; whether he is deliberately set up for that purpose, to adjudicate disputes, or whether we rely upon the decisions of the *de facto* sovereign authority. It will certainly be no use placing any reliance upon the decisions of those who have no power to enforce their decisions, so that if we set up an arbitrator, we must simultaneously surrender our power to him, but if we rely upon the *de facto* authority, then he, of course, already has the necessary power.

What Hobbes then finds it necessary to show, is that there is need of a common standard; that if we leave men to fight between themselves, and to come to various separate agreements, in order to satisfy those of their demands which are backed by sufficient power, the result will be anarchy. And this presents him with the problem, always a difficult one, of distinguishing between the way in which men actually behave in civil society and the way they would behave under conditions which are not at the moment realised, under conditions in his case where there is no central authority. This, as already suggested, is the way in which Hobbes' well-known account of the "war of all against all" is to be interpreted; he is not arguing that there was originally "a state of nature" out of which society developed when men undertook certain contractual obligations (although he often spoke as if this were the case), what he is maintaining primarily is that *if* there were no sovereign authority, then continuous war would be inevitable. The only way to present evidence for a

hypothetical judgment of this kind is to point to tendencies which are operating at any time in society. Hobbes points in the first place to the relations between nations; they are, he says, continually at war with one another, for that uneasy and suspicious peace which we commonly distinguish from war is not worthy of the name peace, and this is solely because there is no common power, to the decisions of which each country could submit in the expectation that other countries will do likewise. Again, in civil war, there is a clear example of the consequences which follow when civil authority is undermined. "It may be perceived", says Hobbes, "what manner of life there would be, where there were no common power to fear; by the manner of life, which men that have formerly lived under a peaceful government, use to degenerate into in a civil war" ("Leviathan", Pt. I, Ch. XIII). Finally, when we perceive the suspicion men betray of their fellow-citizens even where there is a sovereign power, we can see what man's relations to his fellow-men would be, were there no civil society.

These considerations, which serve to indicate that where a sovereign power is absent, anarchy would rule, Hobbes supports by his theory of the nature of man. For his purposes, there are two important forces in human nature, the first the love of power or vainglory, which is the leading anti-social tendency; the second, fear of violent death, and this is the leading social tendency. He supports this account of the matter in the first place negatively, by denying that man is a political animal. He does not, it should be noted, deny that man is born into a society, but he denies that he is born a *member* of society, or what he calls a "true citizen". Clearly, the child is born dependent upon his parent, and dependence is a kind of social relation; but still the child has to be educated to become a member of society, in the sense of a citizen who recognises that he has responsibilies towards society, which must be fulfilled if civil society is to continue. Nor is there any special tendency towards society in human

beings, over and above fear; or at least there is no such tendency which could form the basis of a stable society. He strongly attacks the suggestion that men feel good-will towards their fellow-men as such, and that this good-will is what makes society possible. On the contrary, men are not attracted towards society as such, but towards certain persons, and certain particular social relationships. If we examine closely the nature of a human being's social intercourse, we find he is attracted towards those situations in which he can exercise either his love of power or his love of gain. We seek society in which we can shine in front of our fellow-men; and, if we cannot do this directly, we do so indirectly by depreciating the achievements of others. No doubt, Hobbes says, there may be engendered under these circumstances a kind of "market-friendship", but a society having this as its basis (founded, i.e., upon passions which are competitive in character) can have no stability; and this kind of spurious sociability is all that is involved in what we commonly call "good-will". As a result of this analysis, he concludes, "we must resolve that the origin of all great and lasting societies consisted not in the mutual good-will men had towards each other, but in the mutual fear they had of one another" (loc. cit.).

Only fear, of human passions, is strong enough to overcome the love of power; that is what Hobbes believes to be the outcome of his psychological analysis. If that "perpetual and restless desire of Power" which is "a general inclination of all mankind", were also the *only* human inclination, then we should inevitably have "the war of all against all" as the only condition possible for humanity. It is only because the love of power is constantly checked by fear, that civil society is possible. Hobbes is usually criticised on this point because, it is said, fear is essentially an anti-social, disintegrative force, which would induce men to flee from one another and could certainly not hold them together in society. But, as Hobbes explains in the "Philosophical Rudiments", fear does not

necessarily issue in flight. "To distrust, suspect, take heed, provide so that they may not fear, is also incident to the fearful." It is certainly not the case that we can enter into no social relations with those of whom we are suspicious; the only point is that in such cases we will insist upon some form of independent guarantee, which can only be provided by a sovereign power. In fact, fear may even induce conflict—we may fight because we are afraid as well as in emulation—but just as the society which is the product of pride and gain is of an impermanent kind, so the conflicts of the fearful will not be the basis for a lasting state of war. "Daring to come forth they know each other's spirit"; it will not be long before they see that the others are afraid just as they are, and this will rather strengthen than weaken their allegiance to the social order. When the fearful fight, in fact, the result is either domination (in the case where one is stronger than the other) or compromise—and since compromise, Hobbes insists, has no force without guarantees, in this case, too, the outcome will be acceptance of a sovereign power. That we fear our neighbours, then, while it prevents the establishment of a society in which there is no sovereign power (since that would imply that we could trust our neighbours), will not hinder, but rather encourage, our allegiance to a society in which there is a sovereign power.

The Hobbian position, then, is that it is fear which makes us moral; fear, that is, not of the sovereign power but of what would happen if by constant disobedience we weakened that power. We are not, in modern terms, moral because we are afraid of the policeman, but because we are afraid of what would happen if there were no policemen. If it were not for this fear, our love of power would not permit us to brook the interference of the sovereign; and if there were no sovereign power, there could be no morality. "The Desires and other Passions of man, are in themselves no Sin. No more are the Actions, that proceed from those Passions, till they know a Law that forbids them: which till Laws be made they cannot

know: nor can any Law be made, till they have agreed upon the Person that shall make it. . . . Where there is no Common Power, there is no Law: where no Law, no Injustice" ("Leviathan", Ch. XIII).

There is nothing in this position as we have so far presented it, which would involve any particular theory of social *origin*. Hobbes himself admits that "it may peradventure be thought, that there was never such a time, nor condition of war as this: and I believe it was never generally so, all over the world" (loc. cit.); and, indeed, he constantly argues that there is a distinction between "natural" societies which arise from domination, and "artificial" societies which arise from contract, and in imitation of natural societies. It is only necessary, if his general position is to be established, to argue that a society could not persist if it is founded on pure domination, i.e. that fear of the sovereign as distinct from fear of our fellow-men is never the basis of civil society. He does this when he argues that men are naturally equal, one making up in strength for what another possesses in cunning. Here, without trying to make it a matter of *equality,* we can say that Hobbes is indicating that no person is in himself so superior to other persons that he could hold a whole society in thraldom by his unaided efforts; that in any society which appears to be of such a character, the dominating person must be supported by those who, however hostile they may be to particular acts of the sovereign, yet fear that the result of any move against him would be anarchy, i.e. by those who fear their fellow-men more than their sovereign. Furthermore, just as he argued that only temporary social alliances can be based on good-will, so he would argue equally that no permanent civil society can be founded on pure force. He recognises "duties of a sovereign towards his people", but interprets them in this way; that there are actions of such a kind, that if the sovereign performs them, his society will inevitably disintegrate. If, for example, he arouses in his people a fear of him greater than their

fear of one another, or if he fails in his task of guaranteeing justice (granted that it is his task to determine in the first place what justice consists in), then his rule is bound to come to an end. Thus if we, as citizens, have to surrender much that we would desire as private individuals: if, in particular, we have to recognise that it belongs to the sovereign alone to determine what is to be accounted ours (what is our property) and what is to be accounted due to us, we are in return for this surrender given security from violence directed towards us. (And we must always remember that in "a state of nature" there would be no such thing as property recognised at all—for Hobbes strongly denies that there is anything which could be called a "natural right" to property.)

Granted, then, that there are societies of a kind at any time, the question Hobbes is setting out to answer is this—how it comes about that we give allegiance to the particular society, and the particular code of morals, to which we are accustomed. It is quite irrelevant how our society came into being, whether by force or by contract; what Hobbes desires to put forward is not a theory of origins, but a theory of obligation. His object is to show that if we reject the sovereign power and the moral code of our own community, we have no better alternative to offer; that the only alternative to absolute sovereignty is anarchy, the only alternative to the accepted moral code, another which is equally arbitrary in character. A civil society may change, whether because of sedition, or because the sovereign forgets his duties, or merely on account of an alteration in the character of the sovereign, and with these alterations there will go many in the moral code; but this, Hobbes considers, remains eternally true, that there can be no stable society where there is no undivided sovereignty and where a purely personal moral code is set up against the sovereign will.

What has so far been outlined is that part of the Hobbian theory which is naturalistic in tendency; but it must be admitted that even though Hobbes argues that there can be

societies not based on contract, that the war of all against all need not be regarded as a historical fact, it is still the case that he very often speaks as if all societies *were* founded on contract. Nor is this the only inconsistency in his theory, because although his general position is that in a state of nature "the notions of Right and Wrong, Justice and Injustice have no place", he also speaks as if it were morally right to seek one's own preservation, so that it is not merely that fear makes morality possible, but that it is in itself moral; and, in a parallel fashion, that vainglory is in itself immoral. These inconsistencies in the "Leviathan" have their origin in the fact that the earlier theory of society, which Hobbes presents in the "De Cive", is predominantly rationalistic in character, and that he never quite abandons the doctrines which he there puts forward. What he argues in the first part of the "De Cive", i.e. in what is to be the foundation of his social theory, is that we can discover what is right and wrong by direct appeal to the dictates of Reason as they find expression in "laws of nature", which he in turn equates with moral laws. Now, the first of these dictates is that we should seek peace, and from this first principle it can, he considers, be deduced that we are obliged to surrender our power of deciding what is good and what is evil to the sovereign, so that in the end it is to the sovereign will that we must appeal in matters of morality. But, on this theory, it is *right* that we should thus acknowledge the sovereign rule; and the effects of this way of looking at the matter can be detected in the "Leviathan", also.

It is true that in the "Leviathan" the position of Reason has been much weakened. It is no longer a quite independent power, issuing dictates on its own account, but is, on the contrary, the servant of the passions. The passions (and especially the fear of death) "incline men to peace" and it is at their prompting that reason "suggesteth convenient articles of peace". The "laws of nature" are thus not precepts to which we are obliged to conform, but "theorems" which men

discover "concerning what conduceth to the conservation and defence of ourselves", i.e. they present the means that we will have to adopt if we are to live at peace, and are mandatory only on those who wish to do so. In Kantian language, they are hypothetical, or at most assertorial, and not categorical imperatives. While, however, Hobbes has thus modified the theory of the "De Cive" in a naturalistic way, this is not made sufficiently clear in the "Leviathan", because the later chapters of that book are a briefer version, only slightly altered, of the "De Cive". These alterations represent a most important change in the character of the Hobbian theory, but they are few in number, and we are left as a result with a misleading impression of the argument of the "Leviathan" as a whole. There can be little doubt that however much Hobbes argued that society constituted morality, he could not help feeling that anyone who lacked this fear of anarchy, anyone who preferred war to peace, was morally reprehensible, not merely in so far as he disobeyed the sovereign, but because civilisation was in itself morally superior to barbarism.

Hobbes, then, remains attracted to the contract theory, because he likes to feel that society arose as a deliberate choice on the part of human beings; that it has its source not in anything of a "natural" kind, not, for example, in the family, but in a deliberate preference on the part of human beings. It is then much more plausible to argue that adherence to the *status quo* is in itself moral, because the *status quo* itself had its source in a moral choice (admitting that corruptions have crept in, that through sedition the power of the sovereign has been limited, and that it is then only in so far as the *status quo* is an expression of the sovereign will that we are bound to support it). It is because even in the "Leviathan" Hobbes is trying to have it both ways, to maintain that the sovereign constitutes right and wrong (so that he cannot be impugned on moral grounds) and at the same time that we are right, and not merely prudent, in accepting what

the sovereign decrees, that he does not succeed in shaking himself free of his earlier theory of rights prior to society, and of the social contract. There is, as well, the minor point that he lived in an age in which artificial societies were very much in the air, an age of convention-makers, so that it is inevitable that he should have had an especial interest in "artificial" societies.

It has seemed worth while, not only as a point of scholarship but as bringing to light philosophical problems, thus to disentangle Hobbes' naturalism from his rationalism. For while it is fairly easy to refute his theory of the social contract, it is not so easy to refute his naturalism. In our time, it has become quite clear to what extent people are prepared to surrender liberty to a sovereign power, if they believe that anarchy is the alternative, to what extent security is preferred to freedom. Hobbes has presented a theory which is a standing challenge to democracy, because in maintaining that no society can be stable in which there is not undivided sovereignty, he is maintaining that the liberty democracy allows to institutions to develop their own *mores* can only result in the decay of civil society. This, I should say, is the leading assumption of totalitarianism: that no institution can be permitted independence, that each must be under direct control of the sovereign will. It can only be met by giving a much more exact account than has ever been provided by democratic theory of the relation between institutions and the society in which they appear. It is a challenge to the political theorist as well, to give an adequate account of the distinction between socialising and anti-social forces, between the conditions of cooperation and the conditions of conflict, and to give an alternative treatment of the problem Hobbes is raising—under what conditions civilisation is possible. Finally, it is a challenge to the moralist to show that there *is* a common rule of good and evil, that historicism is inadequate to account for moral distinctions. Whether Hobbes *can* be answered on all these points is another question, but at least we must attempt the task if we are to advance moral and political theory.

PSYCHOLOGY AND OPERATIONISM.

By L. S. Hearnshaw.

I.

In American psychological periodicals of the last few years there have been an increasing number of articles on, and references to, operationism and the operational definition of concepts. Such well-known psychologists as E. G. Boring, J. F. Dashiell and J. R. Kantor, to mention only a few names, express their adherence to operationism. The distinguished surgeon and research worker, Alexis Carrel, when discussing the possibility of a science of man writes: "Operational concepts are the only solid foundation upon which we can build."[1] Gordon Allport in his survey of the trend of American Psychology during the last fifty years notes the recent growing prevalence of operationism.[2] It seems worth while, therefore, to ask what is meant by operationism, and to endeavour to assess its significance for psychology.

Operationism is a methodological principle, and strictly its consideration falls within the province of the philosophy of science. But the time has passed when psychology could ignore problems of methodology. There has been vigorous growth in psychology, during the present century in particular, but this growth has been on the whole an uncritical growth, and, in spite of valuable progress, the consequence has been not only much conflict of opinion and unsound theorising, but also much sterile work and wasted labour.[3] Psychologists,

[1] Alexis Carrel, *Man the Unknown*, 1936, p. 42.

[2] G. W. Allport, "The psychologist's frame of reference", *Psychol. Bull.*, xxxvii, 1940.

[3] Some instances of wasted labour due to false presuppositions are given by H. M. Johnson, "Pre-experimental assumptions as determiners of experimental results", *Psychol. Rev.*, xlvii, 1940.

in their desire to make their subject scientific, have often been impressed with the external features of scientific method, such as experimentation and measurement, rather than with its essentials. The conviction has been growing that the only way out, apart from a slow and wasteful process of trial and error, is through a more thorough understanding of scientific methodology. It is because operationism professes to offer a criterion of sound method that it has won many adherents among psychologists disturbed at the confusion within their science.

II.

The first explicit statement of the operational standpoint occurs in a book entitled "The Logic of Modern Physics" by Professor P. W. Bridgman, Professor of Mathematics at Harvard University. He maintains in this book that the upheaval in modern physics and the shock of relativity were due to an inadequate analysis of the fundamental concepts of physics, and their definition in terms of properties. This mode of definition, which of course had its origin in the Aristotelian canons, is exemplified by Newton's concept of absolute time, defined as that which "from its own nature flows equably without regard to anything external". According to Bridgman a concept must be defined not in terms of properties, but in terms of the operations by which it is made known. In his own words, "the concept is synonymous with the corresponding set of operations".[4] It follows, says Bridgman, from this position that a concept such as that of absolute time is meaningless, because all the operations by which we measure time are relative. It also follows that when we change the operations we change the concept. Thus Bridgman maintains that the concept of length is strictly different when length is determined by direct measurement, by triangulation, and by optical means. The two important points thus seem to be firstly, that concepts are relative, because operations are necessarily relative to a certain set of

[4] P. W. Bridgman, *The Logic of Modern Physics*, 1927, p. 5.

conditions; secondly that concepts are meaningless when a corresponding set of operations cannot be found for them. The mistake of the classical physicists was to ignore the operations by which their fundamental concepts were reached, and to attribute a universal application to these concepts beyond the reach of all possible operations. This mistake can be avoided in future, Bridgman thinks, if all concepts are operationally defined.

In this theory of Bridgman's there are many obscurities and difficulties, and the first of these is as to the meaning of "operation". The definition of "operation" given by S. S. Stevens, the protagonist of operationism in the psychological journals, is not helpful. "An operation is the performance which we execute in order to make known a concept."[5] But we want to know precisely what we are to regard as an operation, or as a legitimate route for making known a concept, and this Stevens's definition does not make clear. In the same article, however, Stevens says that the fundamental operation is discrimination or "the concrete differential reactions of the living organism to environmental states either internal or external". Now it may be true that discrimination is a necessary condition for the formation of a concept, but we can hardly regard it as a sufficient condition, and we are impelled to ask what further operations there are. Professor A. C. Benjamin in his "Introduction to the Philosophy of Science" includes generalising, classifying, ordering, inferring, measuring, pointing, abstracting, constructing, describing, explaining, and negating.[6] To these presumably we may add the more complex experimental techniques used by science. We now have a formidable array of operations. Bridgman himself distinguishes two main classes of operation, physical and mental. C. E. Bures suggests the terminology L-operations

[5] S. S. Stevens, "The Operational Definition of Psychological Concepts", *Psychol. Rev.*, xlii, 1935; see also by the same author "The Operational Basis of Psychology", *Am. J. Psychol.*, xlvii, 1935, and "Psychology and the Science of Science", *Psychol. Bull.* xxxvi, 1939.

[6] A. C. Benjamin, *An Introduction to the Philosophy of Science*, 1937; see in particular Chap. ix.

(i.e. logical operations, including mathematical operations) and P-operations (i.e. physical operations, including observational, manipulative, and instrumental techniques), and he defines an operation as "a logical manipulation or an empirical activity by means of which a concept is used or applied".[7] The question, however, now is, whether the term operation has not become so wide that it is incapable of discriminating valid from invalid concepts. After all even such a concept as absolute time was derived by means of definite operations (abstracting, generalising, etc.) and within the limits of the Newtonian system it proved useful in application. As Crissman, a critic of operationism, writes in a recent article: "If these acts are labelled operations merely to remind us that concepts do not emerge from the void passively freighted with meaning, or if the term merely reaffirms the fact that some reaction by a person or organism is essential to the discrimination of meaning, then the sole cause for wonder is that anyone should think otherwise".[8] The first difficulty, therefore, is that the term operation is not sufficiently precise in its meaning, and does not enable us to discriminate between valid and invalid procedures.

It might be claimed by operationists, however, that the list of operations given by Benjamin is too wide, and that by operations are meant, firstly operations of an observational and experimental nature, and secondly the formal operations of logic and mathematics. From examples given by operationists it would seem that this often is their meaning. For instance when Bridgman asserts that the concept of absolute time is invalid because absolute time can never be measured, he implies that it is only such operations as measurement that are valid. Perhaps this is what Stevens means when he speaks of "concrete operations which can be performed",[9] though it is far from clear which mental

[7] C. E. Bures, "Operationism, Construction and Inference", *J. Phil.*, xxxviii, 1940.

[8] P. Crissman, "The Operational Definition of Concepts", *Psychol. Rev.*, xlvi, 1939.

[9] *Psychol. Rev.*, xlii, 1935.

operations we are to regard as concrete and which not. If, however, this is the meaning of the operationists it seems to land them in an equally difficult position, for it leads to the denial of the validity of generalisation and construction, and it is hard to see how any concepts at all can be reached without these procedures. Operations such as are performed in experimenting and observing are individual and specific, and can never be exactly repeated.[10] To arrive at concepts it is necessary to abstract, to compare, to unite. To deny that these latter are operations is, therefore, to deny the possibility of obtaining concepts. Stevens in his most recent article attempts to meet this point by saying that the criteria for class inclusion are essentially operational tests and refers again to the fundamental operation of discriminating.[11] But discrimination alone cannot give concepts. I can discriminate, let us say, between A and A′, but this does not imply that they belong to different classes, since the difference between them may be irrelevant from the point of view of their classification. Only by the procedure of abstraction, comparison and so on is it possible to arrive at concepts.

Moreover it is hard to deny that there is a valid place in science for what are sometimes termed "constructs", or what Reichenbach terms "illata", i.e. inferred things.[12] Such concepts are not reached by a process of abstraction, but are constructions transcending actual experience. They can be justified not by the operations by which they are reached, but by further consequences that can be deduced from their postulation. Curiously enough Bridgman admits the validity of such constructs which he says "enable us to deal with physical situations which we cannot directly experience through our senses, but with which we have contact indirectly through inference".[13] A construct, he adds, can only be taken

[10] This point is made by R. H. Waters and L. A. Pennington, "Operationism in Psychology", *Psychol. Rev.*, xlv, 1938, and by G. W. Allport, loc. cit.

[11] *Psychol. Bull.*, xxxvi, 1939.

[12] H. Reichenbach, *Experience and Prediction*, 1938; see especially Sections 13, 14 and 25.

[13] Op. cit., p. 53.

as a physical phenomenon when it is connected with other physical phenomena which are independent of those which entered into its definition. For example the concept "stress" can be taken as a physical phenomenon because it is connected with other physical phenomena such as strain and rupture. Thus Bridgman admits that the validity of these concepts cannot be determined by the operations through which they are reached, but only through other operations leading to other concepts which in some way are connected with them. As a result of this discussion, therefore, we are forced to allow firstly, that such operations as abstraction, construction, etc., are inevitable in the formation of concepts, and secondly that the validity of a concept is not necessarily a function of the operations employed in arriving at it. If that is so, the operational definition of concepts does not seem to be a methodological principle of great value.

There is further a difficulty in Bridgman's statement that "the concept is *synonymous* with the corresponding set of operations". It leaves out altogether the referent to which a concept is usually taken to refer. But this leads to curious consequences. If a concept is actually *synonymous* with the corresponding set of operations, there can be no such thing as a mistaken concept unless the operations are in some way misrecorded. So long as the operations performed are correctly recorded, the concept must be valid. Being synonymous with the set of operations, and referring to nothing beyond them, there can be no question, for example, of experimental error, or of inadequate experimentation, nor strictly can there be any point in checking the results achieved by one operational route with those achieved by some other route. In this connection Professor L. J. Russell asks the pertinent question, "How can you speak of the result of the operation as only approximate except in relation to some property you are trying to measure?"[14]

Another objection is that operations seem often to be too abstract to serve as a basis for the definition of concepts.

[14] L. J. Russell, review of "The Logic of Modern Physics", *Mind*, xxxvii, 1928.

The operation of discrimination, for example, of which Stevens makes so much, is highly abstract. The features discriminated are usually far more relevant to the definition of a concept than the operation of discrimination. This is not to deny that the operation may have a bearing upon the definition of the concept; but it is to deny that it is the sole requisite for definition, so that the concept is actually synonymous with the operation. There appears to be much truth in Crissman's contention that the basic confusion of operationism is that "it fails to discriminate the pointing from the thing pointed to".[15]

III.

The task of understanding operationism is made more complex by disagreements among its various exponents, and by the way in which operationism is linked up with other movements such as behaviourism, pragmatism, physicalism, and logical positivism. Speaking of the failure of operationism to bring agreement among psychologists, A. G. Bills writes: "Two of our most articulate apostles of operationism, S. S. Stevens and J. R. Kantor, arrive in recently published articles at diametrically opposed conclusions, as a result of allegedly applying the same operational criterion to their respective concepts. For Kantor comes out with a strict behavioural objectivism; while Stevens on the contrary emerges with a water-tight justification on operational grounds of such ultra-subjective categories as experience, sensation and sensory attribute."[16] Stevens's main work in psychology has been upon hearing and the qualitative analysis of tones,[17] and his justification of this work operationally is worth examining. Stevens agrees with the behaviourists that operations must be "public and repeatable" if they are to be admitted to the body of science.[18]. Therefore "experience must denote some

[15] Loc. cit.

[16] A. G. Bills, "Changing views of Psychology as Science", *Psychol. Rev.*, xlv, 1938.

[17] S. S. Stevens, "The attributes of tones", *Proc. Nat. Acad. Sci.*, 1934; S. S. Stevens and H. Davis, *Hearing: its psychology and physiology*, 1938.

[18] *Psychol. Rev.*, xlii, 1935.

objective performance on the part of human beings". In dealing with what are usually called sensations the objective performances in question are the discriminative language responses of the subject. In other words, if a subject S gives a verbal response a, when stimulated by a stimulus A, and a verbal response b when stimulated by a stimulus B, we are justified in speaking of a qualitative difference, which we may represent by a′ and b′, between the two experiences of S. Now this in effect is precisely what the introspective psychologists have always said. a and b are the introspective reports, A and B the stimuli, a′ and b′ the introspected experiences. And it would usually be agreed that this introspective method is unique to psychology. Stevens, however, denies that there is any uniqueness. "The relation of the psychologist to the object of his investigations is fundamentally not different from that of any other scientist to his subject matter."[19] But if we examine the matter closely this does not seem to be true. Let us take a simple physical experiment, such as weighing an object on a balance. In this case the experimenter X

places the object on the balance—operation O,
observes the pointer reading —operation Q,
reports on the results —operation R.

In the case of a simple psychological experiment, such as observing a pain sensation, the procedure is as follows: the experimenter Y, using S as a subject,

presses a sharp needle on a pain spot—operation O′,
S experiences a sensation —operation Q′,
S makes his report —operation R′.

Now clearly operation Q is public in a sense that operation Q′ is not. Several observers simultaneously may carry out Q: or the pointer reading may be photographed, or otherwise instrumentally recorded. Q′ is essentially private to S. Whereas in the case of the physical experiment all three operations are public, in the case of a psychological experi-

[19] *Am. J. Psychol.*, xlvii, 1935.

ment like the one instanced only the first and third operations are public. We may, it is true, repeat the experiment with other subjects, but in all cases the operation Q′ will be private. No amount of quibbling as to the exact meanings of the words "public" and "private" can do away with the difference between the two types of experiment.[20].

Now Stevens denies this difference, and he does so because he regards language (i.e. the verbal report of the subject) as itself a response to a stimulus, and not merely as a sign of a response to a stimulus. This brings us to the further complication, at any rate in Stevens's exposition of operationism, namely his connection of operationism and logical positivism.[21] Stevens supports his denial of the difference between the physical and the psychological experiment by an appeal to the views of logical positivists, who deny in effect that there is ever a Q operation. According to them in both physical and psychological experiments we have simply the R operations, or, as they call them, protocols. To speak of observations is an incorrect mode of speech, which can be translated into the formally correct language of protocol statements. And these protocol statements can always be reduced to physical language.[22] So we find that Stevens, though in fact he admits the type of experiments and the methods called as a rule introspective, asserts that psychology must use a behaviouristic approach. We should probably interpret Stevens rightly by saying that though he admits that the traditional psychological terms, such as experience, sensation, etc., may still be used, he is really interpreting these terms differently, and using them to refer not to subjective experience, but to certain types of observable reactions, chiefly language reactions. In effect, therefore, operationism is seen to be allied to movements, such as behaviourism and logical

[20] The "public tests", for example mentioned by Professor J. N. Findlay ("Some reactions to recent Cambridge Philosophy, I", *Aust. J. Psychol. and Phil.*, xviii, 1940, p. 207) do not alter the status of the thing being tested. The tests are at best very indirect.

[21] *Psychol. Bull.*, xxxvi, 1939.

[22] For a brief exposition of these views see R. Carnap, *The Unity of Science*, 1934.

positivism, which deny immediate experience. This we might have guessed from the first, for in asserting that concepts could not be defined by means of properties but only by means of operations, the operationists were tacitly denying the existence of properties or qualitative determinations.

We now see rather more clearly where operationism stands. It is a sub-movement within the wider movements of logistic empiricism and behaviourism. Operationism, however, is a movement which has arisen within science itself, not within philosophy. Bridgman is a mathematical physicist, and Stevens an experimental psychologist. This perhaps explains the indefiniteness of operationism and its undeniable lack of clarity. It is because of this indefiniteness and lack of clarity that operationism as it stands cannot, in my judgement, be of much value to the psychologist. The attempt to bolster up operationism by equating it with logical positivism will not add to its attractions in the eyes of most psychologists, for logical positivism, which is almost obsequiously servile to the physicist, is dictatorial in its prescriptions to the psychologist, and dogmatises on matters which, it seems to the psychologist, can be decided only empirically, and only when our knowledge has advanced much further.[23] Nevertheless the aim of operationism, to provide "a procedure by which the concepts of psychology can be cast in rigorous form"[24] is a laudable one, and we may perhaps conclude this article by suggesting briefly an alternative method by which this aim may be carried out.

IV.

Concepts are derived from experience by the processes of abstraction, synthesis, and construction. These processes can be reduced in their simplest terms to the noegenetic laws of

[23] Cf. R. Carnap, op. cit., p. 71: "The definition of any psychological term reduces it to physical terms." Such a reduction is at present purely hypothetical, and to assert it dogmatically prejudges some of the most important issues in psychology.

[24] S. S. Stevens, *Psychol. Rev.*, xlii, 1935.

Spearman.[25] Concepts on their formation become symbolised by words, and words have a functional unity which is often mistaken for the functional unity of their referents.[26] Words become used in a large variety of contexts, and the grasp of the context on each occasion enables them to be used significantly in practical life without undue confusion. But the referents which they stand for need have no functional unity, and indeed are often related by no more than superficial analogies. The task of science is primarily the ascertaining of functional relationships, and hence the concepts employed by science must refer to referents which are functional unities within the level of reference being symbolised at the time.[27] The critique of concepts must, therefore, be a functional one. But a concept which refers to what can be taken as a functional unity in one type of situation or context, need not when the type of situation or context is changed. For this reason all concepts are relative to types of situations or contexts. The operationists are perfectly correct in insisting on this relativity; they are wrong in saying that the relativity is to the operations of the knower alone, and not to the whole situation or context. In criticising concepts, therefore, we must ask two questions: firstly, does the concept refer to a functional unity? (if not, the concept is for scientific purposes, though not necessarily for limited practical purposes, valueless): secondly, within what type of context was this functional unity established? (And the implication is that, without proof, the concept may not be used outside the same class of context.) Concepts are of three main kinds: those referring to concreta, those referring to abstracta, and those referring to "illata". The functional unity of the referents is established in each case rather differently.

The task of science is, needless to say, not completed with the establishment of valid concepts. We need to know

[25] C. Spearman, *The Nature of Intelligence and the Principles of Cognition*, 1923, Chaps. iv-viii and xvi.

[26] This term is borrowed from C. K. Ogden and I. A. Richards; see *The Meaning of Meaning*, 1923, pp. 14 and 15.

[27] See Ogden and Richards, op. cit., pp. 192-4.

as fully as possible the conditions upon which the functional unities referred to depend, and the laws governing their interrelationships. Now the establishment of such functional unities and relationships obviously cannot result from superficial observation or the type of reflection which rests content with division, classification, and the drawing of analogies. It can result only from the study of action and reaction either by directed observation over a period of time, or by experiment, assisted in each case by an accurate logical and mathematical technique. This, we suggest, is in brief the fundamental basis of the critique of scientific concepts.

These remarks may be illustrated from the field of psychology. Take for instance the faculties of the old faculty psychology, symbolised by terms such as "intellect", "memory", "imagination", "attention". The main objection to this faculty psychology is, as Spearman points out, that it "assumed that community of title involved concurrence in function".[28] There may be perhaps superficial analogies between the various mental processes for which the word "imagination" is taken to stand, but no one could now claim that there is any functional unity among them. It is the aim of the factor method, which Spearman was the first to use in psychology, to ensure that the concepts employed by psychology refer to functional unities,[29] and the vast importance of the use of statistical techniques in psychology is that these methods are a powerful tool in indicating the presence or absence of such functional unities.

The unsatisfactory state of the psychology of personality is due largely to the fact that there is no guarantee that the trait names used in the description of personality refer to functional unities. The naïve belief of Allport that trait names refer even approximately to real constituents in personality seems a groundless assumption, and the prodigious labour involved in the collection from the dictionary of 18,000 such names, in the hope of advancing the scientific study of

[28] C. Spearman, *Psychology down the Ages*, 1937, Vol. ii, p. 188

[29] C. Spearman, *The Abilities of Man*, 1927.

personality, seems almost comic.[30] In the first place a trait name like sociability has various meanings often only superficially related (one man may be most sociable among a group of intimate friends, but unsociable to strangers: another man may be sociable to strangers, but selfish and unreasonable among friends: yet both may be termed, in different senses of the word, sociable); in the second place there is no reason to assume without proof that the sociability of any individual on different occasions and in different circumstances is the manifestation of a unitary trait. Of course, for the practical purposes of everyday life, and also for certain kinds of psychological description, the use of these trait names is justified; but when they are taken out of these contexts, and are used for the purpose of establishing a science of personality, their scientific inadequacy becomes apparent.

The same criticism applies to McDougall's list of instincts. What right have we to assume that there is a functional unity behind, let us say, the various acts of acquisition performed by human beings, and hence to postulate an acquisitive instinct? Unless functional unity is proved, to postulate such an instinct is merely to give a pseudo-explanation.[31] To define an instinct with McDougall as a "psycho-physical disposition" is virtually meaningless, because the word "disposition", which equals only "arrangement", refers to nothing definite at all. Functional unity must in all cases be proved before a concept is more than hypothetical, and it can be proved only by experiment, or by controlled observation assisted by statistical techniques.

V.

The operationists are perfectly correct in insisting that a far more rigid critique of the concepts used in psychology is

[30] G. W. Allport, *Personality*, 1937, Chap. xi; G. W. Allport and H. S. Odbert, *Trait Names: a psycho-lexical study*, 1936.

[31] C. A. Mace ("Faculties and Instincts", *Mind*, xl, 1931) maintains that McDougall's doctrine of instinct avoids this error. It may be that some of McDougall's instincts could be validated, but not his list as a whole. McDougall's criteria for distinguishing instincts are inadequate, and he has not realised the need for proving functional unity.

necessary. Their operational criterion fails firstly because the term "operation" lacks precision, and secondly because they assert that concepts are synonymous with operations. Stevens endeavours to add precision to operationism by allying it with logical positivism. But this involves the denial of immediate experience and the qualitative aspects of experience, and is not a view which will commend itself to many psychologists other than behaviourists. We need a theory which, while providing a basis for the critique of concepts, will be acceptable to a wider circle of psychologists, and does not prejudge important issues. The view put forward in the last section endeavours to outline such a theory. It agrees with operationism that concepts can only be justified pragmatically, because only by controlled observation and experiment can functional unities and functional relationships be established. But the functional unities and relationships, though necessarily relative to, are not primarily determined by, the operations through which they are ascertained. Hence firstly not all operations are valid, and secondly it is incorrect to say that the resulting concepts are synonymous with the operations, or can be defined solely in terms of operations.

IS THERE A GENERAL FACTOR OF PERSEVERATION?[1]

By K. F. Walker, R. G. Staines and J. C. Kenna.

Since the earlier studies of the Spearman school, which established the existence of "g", various other group factors independent of "g" have been distinguished, notably in the field of personality traits. Of these "p" factor has had the widest practical application, and promises to develop into the most useful diagnostic weapon.[2] In this paper the evidence for the existence of such a factor is reviewed, and Spearman's theories of its nature discussed. The two issues are logically separable, but Spearman has bound them together in a well-knit doctrine in which "g" is taken to measure the quantity of "mental energy", and "p" its degree of inertia, both of these being subject to individual differences. By the inertia of mental energy he means that "cognitive processes always both begin and cease more gradually than their (apparent) causes",[3] and he asserts that the phenomena of perseveration are instances of this general law *because* they give evidence of a general factor participating in all of them. The whole theory of mental inertia thus rests on the premise that there is a group factor "p", to the operation of which all particular instances of perseveration may be attributed. If this premise

[1] Our thanks are due to Professor H. T. Lovell and Dr. G. Phillips for advice and criticism, and to Miss N. Hamilton, B.A., for making available to us the raw scores of her investigation, cited in the text.

[2] Spearman shares this view. He writes: "When the perseveration . . . has been evaluated for persons of diverse age, sex, character, and social status; when the connection has been traced out which it bears to success in different branches of education and varieties of vocation—then perhaps psychological science will have made a second advance not much less in magnitude than that which is being achieved with respect to 'intelligence' " (*The Abilities of Man*, Macmillan, London, 1927, p. 307).

[3] Op. cit., p. 291.

is not true, "perseveration" cannot measure the inertia of the nervous system, for such an inertia must operate in all behaviour. The value of "perseveration" as an explanatory concept would be gone. We should simply have to talk of individual differences in perseveration of particular activities.

The Range of Spearman's Observations.

The first problem is the definition of perseveration, for until we know what phenomena are to be included in that category, we cannot be sure that our test batteries include all instances of perseveration. Unless they do, we cannot argue that there is a group factor operative in all varieties of what is called perseveration. In the past the term has been very widely applied, and Spearman can attribute all the phenomena, which from time to time have been subsumed under the concept of perseveration, to "p" factor only if his quantitative data include measurements of all of them.

Spearman's data omit many of the phenomena that have been instanced as "perseverative" since Neisser first used the term in 1894 to describe the clinical symptom of "abnormally persistent repetition or continuation of an activity after the activity had been once begun or recently completed".[4] Notice that two processes are described in Neisser's definition—*continuation* and *repetition*. When Müller and Pilzecker[5] postulated a perseverative tendency to account for certain phenomena of memory in normal individuals, they also included both these processes. But they added a third type of process, *interference* of one activity with another initiated soon after it.

The quantitative data whose statistical treatment according to Spearman yields the group factor "p", consist only of tests of continuation and interference with the exception of the questionnaire used by Lankes. Wynn Jones used the following tests:

[4] W. S. Foster, "On the Perseverative Tendency", *Amer. J. Psychol.*, XXV, 1914, pp. 393-426.

[5] G. E. Müller and A. Pilzecker, "Experimentelle Beiträge zur Lehre vom Gedächtnis", *Zeitschrift f. Psychol.*, Ergbd. 1, 58 et seq., 1900.

(1) Writing an S, first in the usual way, then as it would appear in a mirror.
(2) Writing digits, first in the usual way, then by making the stroke backwards.
(3) Mirror drawing, when the design to be copied is seen only in a mirror.
(4) Copying prose, first in the usual way, and then without dotting "i's" or crossing "t's".

In each case the measure of perseveration was the difference between the score for the usual, and that for the unusual, activity. Where are the measurements of the phenomena referred to by Müller and Pilzecker? All that is measured here is interference; continuation and repetition are ignored. When after reviewing this study, Spearman says, "Now at last, then, the evidence for some group factor or factors pervading these tests of perseveration leaves nothing to be desired", the word "these" should be italicised, for the group factor is restricted only to phenomena of interference.

In Lankes'[6] study two tests of continuation were used, the natural rate of tapping, and the flicker test for colour fusion. In the first the subject was told "simply to move the finger, each subject at his own rate, just as he feels it natural to himself at the time". In the flicker test a revolving colour disc was used, the two sectors of which were complementary in colour, so that when rotated at a sufficient speed they merged into a uniform grey. It was argued that the speed of rotation which was just necessary to produce the fusion measured the degree of perseveration, the high perseverator experiencing a longer after-image of each colour, thus producing the fusion at a lower speed of rotation. It was argued that the slower the natural rate of tapping, the higher the perseveration.

All the other tests in Lankes' battery were tests of interference, with the exception of his questionnaire, reproduced in full in the Appendix to this article. If this questionnaire

[6] W. Lankes, "Perseveration", *Brit. J. Psychol.*, Vol. VII, 1915, pp. 387-419.

be examined, it will be found that out of the twelve questions finally used five (Nos. 1, 2, 6, 10, 12*b*) could be conceived to refer to *repetition* of activity, seven (Nos. 3, 4, 5, 6, 10, 11, 12) to *continuation* of activity, and four (Nos. 9, 7, 5, 8) to *interference* effects. And yet the total score for this questionnaire was simply the sum of all items marked so as to indicate high perseveration.

Other questions were omitted from the interrogatory when it was discovered that they did not correlate with the pooled tests, the replies of high perseverators on the tests being no different from those given by low perseverators. By taking their correlation with his test battery as his criterion of whether the various items all refer to the same thing, Lankes is assuming away what he set out to investigate. If questions tapping other phenomena which used to be considered "perseverative" do not correlate with his test battery he *assumes* that they cannot tap "perseveration". But this is precisely what he is trying to discover—whether all these phenomena *do* intercorrelate. His criterion of "perseveration" appears to be his tests, which, as we have seen, are almost without exception tests of interference only.

No other investigator of the Spearman School cast his net as widely as Lankes, and *quite apart from the validity of his statistical argument,* it must be concluded that Spearman's data do not indicate the functional unity of all the phenomena that are designated as perseverative even by Müller and Pilzecker, let alone the additional phenomena so designated by later writers.

It is necessary, therefore, to restrict our interpretation of Spearman's account of the perseverative tendency to the phenomena measured in his data. These are interference effects and continuation effects. One function which Müller and Pilzecker attributed to the perseverative tendency, that of causing mental contents to return spontaneously (i.e. without associative induction) to consciousness, is not included in Spearman's concept, which refers only to continuation of activity, and its interference with later activities.

Spearman subsumes repetition under his "law of retentivity", which states that "cognitive events by occurring establish conditions which facilitate their recurrence"[7] and classes its inclusion with continuance and interference as an error second only in seriousness to linking perseveration with steadfastness of purpose.[8] It is important to emphasise that Spearman's concept is more limited than that of Müller and Pilzecker, for the term "perseveration" is still widely used to refer to the spontaneous recurrence of ideas in consciousness[9] and to the repetition of an activity against the subject's will.[10] Also we find one of Spearman's pupils, R. B. Cattell, citing the recurrence of a tune to the mind as an instance of perseveration[11] although it is a case of repetition, and explicitly excluded from Spearman's concept.

The Statistical Evidence.

The first study which presented statistical data bearing on the existence of a general factor of perseveration was that of Wynn Jones,[12] who administered the four tests described above to 77 children, averaging 12 years of age. Spearman's statistical treatment of the results leaves him convinced that they can be accounted for only on the hypothesis of a group factor, over and above "g", common to these tests. But, as Burri has pointed out,[13] the argument is not convincing. Spearman applies his customary "reference test" to the data

[7] Op. cit., p. 271.

[8] Ibid., p. 306.

[9] I. Kendig and B. Shevach, "Studies in Perseveration, I-IV", *Journ. of Psychol.*, Vol. III, pp. 223-64.

[10] E.g., D. Henderson and R. Gillespie, *A Textbook of Psychiatry*, London, 1927, p. 87.

[11] R. B. Cattell, *Your Mind and Mine*, Harrap, London, 1934, p. 224. Other pupils of Spearman, such as Wynn Jones, are more cautious. In his *Theory and Practice of Psychology* (Macmillan, London, 1934) Jones writes: "More investigations are desirable in order to ascertain the relations between perseveration as operative in tests such as have been described, and perseveration as defined by Neisser, G. E. Müller, Wiersma and others" (p. 193).

[12] L. Wynn Jones, "Perseveration", *Brit. Assoc. Sc.*, 1915, pp. 698-9.

[13] Clara Burri, "The Present Status of the Problem of Individual Differences in Alternating Activities", *Psychol. Bull.*, Vol. 32, 1935, pp. 113-139.

(described in the Appendix to *The Abilities of Man,* p. xxiii), thus first measuring the amount of "g" in each of the tests, and then by partial correlation discovering the extent of the relation between the various tests over and above the relationship due to "g". Any relation that is so left over may be attributed to group factors other than "g". One tetrad difference is given (·399±·048) from two "reference abilities" and two perseveration tests (intercorrelation +·6), which being seven times greater than its P.E., cannot be attributed to sampling. Burri goes on to say, "Granted that according to this analysis, a group factor between the two perseveration values is present, these findings from one tetrad difference do not permit one to draw any generalisations. Why does the report give only one tetrad difference when three were possible, and specific correlations for only two tests when four were used and thus six combinations could have been made? All the tetrad differences from these tests should be considered in reaching any conclusions even for these two tests. Furthermore, Spearman discusses Jones's results in such a way as to convey the impression that the finding of this group factor proves that it is perseveration. If we study the logic of the tetrad theory, however, all that may be concluded from the analysis is that the tetrad equation is not satisfied and that the tetrad difference is more than five times its probable error, which would indicate that the two factor hypothesis does not hold, and that there is not only one common factor to account for the intercorrelations, but that one or more group factors are present. These results do not show that there is only one additional factor and that this factor is perseveration" (pp. 129-130).

Spearman's inconsistent treatment of the data comes out still more clearly in another of his pupils' studies, Pinard's investigation of perseveration in problem children.[14] In this case the tetrad equation is satisfied, and the group factor thus discovered is called perseveration. But if we apply the same

[14] J. W. Pinard, "Tests of Perseveration (their relation to character)", *Brit. J. Psychol.,* 1932, Vol. 23, pp. 5-19.

procedure as Spearman applied in the case of Jones's results, it should be called "g". Of course, as Burri points out,[15] if the tetrad theory be taken as a purely mathematical theorem, then Pinard is justified in calling whatever group factor he finds anything he likes. But surely if this group factor is to be called "p", and not "g", it is just a matter of personal preference whether we call a group factor "p", "g" or any other letter of the alphabet. Another difficulty is how Pinard, using very similar tests[16] to those of Jones, found the tetrad equation satisfied and Jones did not. However it happened, these opposed results cannot both be quoted as evidence for a general factor of perseveration.

The most ambitious study of perseveration as a general factor that has been undertaken by a member of the London School is that of Lankes.[17] The intercorrelation of his various tests range from −·50 to +·50, averaging +·41. No tetrad analysis is reported, but the results are attributed to a general factor of perseveration, since the tests were constructed so as to eliminate "g". Kelley[18] argues that the postulation of a general factor is unwarranted, as the intercorrelations are by no means large enough to be unequivocal, especially with such a small group (N= 47). In any case, might not such a general factor be "g"?

The same criticism applies to two other studies with similar results, those of Bernstein [19] and Clarke,[20] though their groups were larger. No tetrad analysis is reported by either investigator. Each proceeds on the assumption that a general factor of perseveration exists and that it is independent of "g". We have seen that this assumption is not warranted,

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[15] Op. cit., pp. 130-1.

[16] Pinard used (a) the inverted 'S' test; (b) the inverted triangles test; (c) the "alphabet-number" test; (d) the mirror-writing test.

[17] Op. cit.

[18] T. Kelley, *Crossroads in the Mind of Man*, Stanford Univ. Press, pp. 20-1.

[19] E. Bernstein, "Quickness and Intelligence", *Brit. J. Psychol. Monog. Supp.*, III, 1924, 72.

[20] G. Clarke, "Some Character Traits of Delinquent Normal Children in Terms of Perseveration", *Aust. Counc. Educ. Res. Publ.*, No. 29, 1934.

and the size of the intercorrelations is not sufficient independent evidence of a general factor other than "g".

It should also be noted that one investigator of the London School, Hargreaves,[21] found no evidence of a general factor in his results from the following battery of tests:

(*a*) Towns test.
(*b*) Word-building.
(*c*) Tapping.
(*d*) Reverse stroke (writing 2 backwards).
(*e*) Inverted "S".
(*f*) "it" test (crossing "t's" and dotting "i's").

The most comprehensive investigation is that of Jasper,[22] which yielded no evidence of a general factor other than "g". He employed a wider range of tests than any British investigator has used. His main group of subjects numbered 56. As measures of "sensory perseveration" he used the following tests:

(*a*) Light adaptation. (Score is time required to see a light of constant low illumination after being adapted to 100 watt floodlight; the lower the score, the lower the perseveration.)
(*b*) Mean simple reaction time to visual stimuli (20 measures).
(*c*) Mean choice reaction time to visual stimuli (20 measures).
(*d*) Sigma simple reaction time to visual stimuli.
(*e*) Sigma choice reaction time to visual stimuli. (It was assumed in (*d*) and (*e*) that the low perseverator would be more variable in reaction time than the high perseverator.)

As measures of "motor perseveration" the following tests were employed:

(*f*) Tapping rate.

[21] H. L. Hargreaves, "The 'Faculty' of Imagination", *Brit. J. Psychol. Mon. Suppl.*, 1927, No. 10.

[22] H. H. Jasper, "Is Perseveration a Functional Unit Participating in all Behaviour?", *J. Social Psychol.*, Vol. II, 1931, pp. 35-51.

(*g*) Star tracing in mirror vision.
(*h*) Motor inhibition (the power to inhibit a learned finger-reaction in response to visual stimuli).
(*i*) Motor interference (interference with learned finger-reaction after test *h*).
(*j*) Changed order letters (Lankes' test).
(*k*) Cancellation (Lankes).
(*l*) Arithmetic shift (from adding to multiplying).
(*m*) Arithmetic shift (from subtraction to division).
(*n*) Narrative memory test (immediate recall).
(*o*) Narrative memory test (difference between immediate and delayed recall).
(*p*) Questionnaire. (Even more catholic in range than that of Lankes).

No evidence of a general factor is obtained, and it must be admitted that Jasper's investigation is at least as impressive as that of Lankes. Jasper is not convinced that "p" tests measure "perseveration", and presents other criticisms of the London school's statistical evidence which cannot be gone into here.

Two other investigations yielded no general factor. In an unpublished study[23] for the Department of Psychology in the University of Sydney, Miss N. Hamilton administered a battery of five motor perseveration tests to a group of 50 schoolboys.

The following intercorrelations were obtained:

Table I.

Test.	1.	2.	3.	4.	5.
1		−07	+22	−10	−06
2			−14	+01	+05
3				+11	+09
4					+25

These correlations are not significant, and a tetrad analysis yielded no group factor.

[23] N. Hamilton, "Perseveration and Stability in School Children", unpub. thesis, Dept. of Psychology, University of Sydney. Miss Hamilton's tests were: (1) Dot-dash test. (2) ABC, abc test. (3) Reversed "S" test. (4) Reversed "2" test. (5) Reversed "W" test.

The present writers administered a battery of six motor perseveration tests to a group of 205 University students. This appears to be the largest group to be tested for perseveration, yet no general factor was obtained, as a tetrad analysis of the following intercorrelations showed:

Table II.

Test.	1.	2.	3.	4.	5.	6.
1		+·08	+·18	+·24	+·17	+·02
2			+·15	−·03	+·07	+·09
3				+·02	+·23	+·17
4					+·17	+·05
5						+·05

It may be concluded, then, that the statistical evidence for a general factor of perseveration is as yet by no means satisfactory. We have already emphasised that Spearman's law of inertia does not subsume all the phenomena that from time to time have been called "perseveration". Altogether, then, the theory of "p" factor is in parlous condition. But Spearman's hypothesis of "mental inertia", which he puts forward as a tentative explanation of "p" factor, is also open to question, even if the evidence for the existence of such a factor were perfectly satisfactory.

Perseveration Tests and Mental Inertia.

Spearman holds that each individual possesses a certain amount of mental energy ("g") which possesses an inertia analogous to that of physical energy, so that changing swiftly from one activity to any other activity causes a certain interference with the second activity, as long as the "lag" or "perseveration" of the first activity persists. This interference has been measured in various ways. Before Pinard's investigation, it was customary to measure interference by comparing the subject's performance of an old, well established activity with his performance of a new, conflicting activity following immediately upon the old. This type of test involves what Cattell calls "creative effort", the breaking down of an old

habit in the learning of a new.[24] It may be doubted whether this is the same thing as the lag of a cognitive process, the inertia of mental energy. Surely people may be expected to write less "S's" backwards than forwards at any time, whether they have just been writing them forwards or not. Is it not likely that the interference score obtained by comparing an individual's performance in writing forward "S's" with his performance in writing them backwards, contains interference due to the conflict of the two habits, as well as interference resulting from the lag of the tendency to write forward "S's"? If this is so, scores on perseveration tests of this type could hardly be pure measures of mental inertia as defined in Spearman's law of inertia, for they also include interference resulting from the rigidity of the old habits, quite independently of the temporal sequence of the two activities.

In an attempt to test this possibility, Cattell first administered a battery of perseveration tests in the usual way, the old activity preceding the new, obtaining a score he designated $\frac{X_1}{Y_1}$. Then the same tests were administered with the new activity preceding the old, giving a score he designated as $\frac{X_2}{Y_2}$. He took as his final score $\frac{X_1}{Y_1} + \frac{X_2}{Y_2}$, which should give a measure of interference independent of the difference in difficulty of the two activities, i.e. independently of the retentivity of the old disposition. When scored in this way, however, the general factor which he had previously found on the usual method of scoring disappeared. From this Cattell drew the inevitable conclusion that perseveration tests do not measure a general mental inertia "independent of whether the change is from old to new or new to old habits". When the tests were re-scored by changing the sign of the second part, so that the final score became a measure of the relative performance in the two activities independently of this temporal position, still no general factor appeared.

[24] R. B. Cattell, "Temperament Tests, II", *Brit. J. Psychol.*, XXIV, 1933, pp. 20-49.

The position is, then, that perseveration tests (of the creative effort type) give a general factor when the old activity precedes the new, and the score measures interference resulting from the rigidity of the old habit as well as from the lag of the first (the old) activity. When the interference resulting from the rigidity of the old habit is eliminated, no general factor appears. Whatever the general factor may be that is operative in creative effort tests of perseveration, it is not mental inertia. But Cattell refuses to admit this and argues that it is in these tests that "inertia perseveration" is most clearly shown! He starts from the premise that there is a general factor of inertia perseveration, and when tests which give a pure measure of inertia perseveration do not indicate a general factor he argues that inertia perseveration appears most clearly in tests which include other factors! The inconsistency of this argument is obvious, and it is clear that whatever general factor may be yielded by creative effort tests, it is not one of mental inertia as defined by Spearman. (We have already argued that there is little evidence of a general factor other than "g".)

"Creative efforts tests" may be contrasted with "alternation tests" first introduced by Pinard, and soon generally adopted. In this type of test the interference resulting from perseveration is measured by comparison of the subject's performance of two activities independently, with his performance of them in rapid alternation. Thus the "reversed S" test in its alternation form, consists of:

(*a*) Writing S forwards for 30 secs.
(*b*) Writing Ƨ backwards for 30 secs.
(*c*) Same as (*a*).
(*d*) Same as (*b*).
(*e*) Writing SƧ for 30 secs.
(*f*) Same as (*e*).

The interference score is then taken to be $\frac{(a+b+c+d)}{2(e+f)}$. It is argued that the high perseverator will find more difficulty in swift alternation of the two activities.

Cattell[25] claims that "creative effort" tests and "alternation" tests measure the same thing, since he obtained fairly high intercorrelations in a battery containing both types of test. But if there is an initial difference in the performance of the two activities, there is bound to be high correlation owing to the time allocation of the alternation test. In summing (a+b+c+d), we have the score for a total period of two minutes, of which exactly one minute has been devoted to S, and one minute to ♀. Let us suppose that the following scores are recorded in the first part of the test:

(*a*) 30S.

(*b*) 15♀.

(*c*) 30S.

(*d*) 15♀.

In one minute, 60S are made; in the same time only 30♀ are made. Each ♀ has taken just twice as long as each S. If there is no change at all in speed in either activity, in the alternation part of the test, each S♀ unit will take (1+2) seconds to make. In (*e*) and (*f*) we have a period of 1 minute, and we double the number of S's and ♀'s done in this time. If it takes 3 seconds to do one S and one ♀, when alternated, then the total score in one minute is 40, which gives 80 when doubled. Perseveration score is then $\frac{90}{80}$, showing some interference, although we have made no allowance at all for difficulty in alternation! The time is so allocated in the alternation test that an interference score is always obtained, whether or no the subject has any difficulty in alternation. This interference is always proportionate to the initial difference between the two activities, for the greater this is, the more the faster activity is slowed down by having to alternate with the slower activity. In the above instance, if the average time for ♀ in the first part of the test were 3 seconds instead of 2, each S♀ unit would take (1+3) seconds

[25] R. B. Cattell, "On the Measurement of Perseveration", *Brit. J. Educ. Psychol.* Vol. V, 1935, pp. 76 et seq.

and the subject would do 15 of them in a minute, giving a final score of $\frac{60+20}{2(30)} = \frac{80}{60}$.

The score of an alternation test is thus not a pure measure of difficulty in alternation at all. It results also from whatever initial difference exists in the performance of the two activities. If a pure measure of difficulty in alternation is desired, the influence of initial difference must be eliminated, as was done by Clarke.[26] The method employed by us is somewhat simpler, however.[27] By the preceding analysis it is possible to forecast the score of each subject on (e+f) from a knowledge of his scores in the first four parts of the test, assuming that he has no difficulty at all in alternating. The difference between this "expected" score and his natural score on (e+f) measures his difficulty in alternation. The alternation score is given by $\frac{\text{Expected (e+f)}}{\text{Actual (e+f)}} \times 100$. The "expected" (e+f) is calculated as follows:

$$\text{Expected (e+f)} = \frac{60}{\text{Time for one S} + \text{Time for one } \delta}$$

$$= \frac{60}{T_1+T_2}$$

$$T_1 = \frac{60}{(a+c)}$$

$$T_2 = \frac{60}{(b+d)}$$

The simplest way of obtaining a pure alternation score would be to set the number of items to be completed in each part of the test, and score by recording the time required.

When a battery of alternation tests which yields a general factor on the usual method of scoring is re-scored in this fashion to give a pure alternation score, the general factor

[26] Op. cit., pp. 25-6.

[27] This method was described in Miss Hamilton's thesis, and was first suggested by Dr. A. H. Martin, of the Dept. of Psychology, University of Sydney.

disappears. In her unpublished study for the Department of Psychology, Sydney University, Miss N. Hamilton also administered five alternation tests to a group of 75 boys aged 12 to 14, ranging from 90 to 110 I.Q., who were educationally retarded, in most cases as a result of some difficulty in adjustment. When scored in the usual way, the following intercorrelations were obtained:

Table III.

Test.	1.	2.	3.	4.	5.
1		+·71	+·72	+·56	+·71
2			+·75	+·64	+·71
3				+·55	+·64
4					+·78

These correlations satisfy the tetrad equation and thus give evidence of a general factor. Leaving aside the question whether this factor is "g", there are two possibilities. One is that it is a general factor of alternation difficulty, presumably due to an inertia perseveration. The other is that the intercorrelations are high because all tests show large initial differences between the old and the new habits.

When the tests were re-scored by the writers to give a pure measure of alternation difficulty, the following intercorrelations were obtained:

Table IV.

Test.	1.	2.	3.	4.	5.
1		+·31	+·37	−·04	+·04
2			+·27	−·19	+·08
3				+·14	+·14
4					+·23

The majority of these correlations are not statistically significant, and they do not yield a group factor. It follows that whatever the group factor yielded by the usual method of scoring may be, it cannot be a general factor of mental inertia, for when the tests are scored so as to eliminate all other influences except inertia, as Spearman defines it, no general factor appears.

CONCLUDING DISCUSSION.

Altogether there is little evidence of a general factor of perseveration, even as defined by Spearman, who restricts the concept to continuance and interference effects, thus excluding the repetition which from time to time has been instanced as perseveration. The statistical evidence is meagre and inconsistently treated.

Even if this evidence left nothing to be desired, the general factor would not be one of mental inertia as defined by Spearman, because the tests used do not give a pure measure of inertia, but include the influence of the rigidity of old-established habits in face of conflicting new ones. When this influence is eliminated, what statistical evidence there is for a general factor vanishes.

It might be argued that the inertia of a habit is proportionate to its rigidity, so that the difficulty of forming a new conflicting habit is a measure of the inertia of the old. This view has had a long history as the concept of "primary memory", "the inertia of the nerve substance", which according to James is "the first manifestation of elementary habit",[28] but it is not consistent with Spearman's usage of the term.[29] "Habit inertia" takes effect quite independently of the temporal position of the two activities. We find it harder to write a reversed "S" than an ordinary one *at any time,* though we may find it still more difficult if we have just been writing ordinary "S's". But Spearman's law of inertia refers only to the difficulty engendered by the temporal succession of the old and the new activities; "habit inertia" refers to the difficulty encountered independently of temporal succession. In perseveration tests as ordinarily administered and scored, the two phenomena are mingled indiscriminately. When they are separated, Spearman's law of inertia, resting as it does on flimsy statistical foundations, finds little empirical support.

[28] W. James, *Principles of Psychology,* Macmillan & Co., London, 1890, Vol. I, p. 646.

[29] "How", he writes, "shall such a 'lag' of a process . . . be distinguished from the after 'disposition'? In truth, the differences between the two appears (*sic*) ever wider and deeper the more profoundly it is examined" (*The Abilities of Man,* p. 291).

Appendix.

Interrogatory on Perseveration-tendency employed by Lankes. (Cited by C. Spearman, *The Abilities of Man,* pp. 299-300. Other questions were omitted on account of low correlation with the test battery.)

Read the following questions several times at your leisure, and write a short, but clear, answer (simply a "yes" or a "no", "very much", "never") on the respective line.

1. Do you often notice a tune, line of poetry, phrase, problem, etc., coming back to your mind again and again without your intending it? How often (about) a week? At what time of the day more frequently?
2. Are your dreams more commonly about some past experience or events? Or rather about things scarcely ever thought of before?
3. When something is to be done or imminent, e.g. a task, an examination, etc., does it often come to your mind during the days preceding it?
4. When writing an essay, or working out a problem, do you find it easy to interrupt it? Or do you feel a strong tendency first to finish it in spite of fatigue?
5. When you have to interrupt it, does your attention easily pass on to other things? Or do the thoughts of the essay or problem keep coming back to your mind?
6. On taking it up again after the interruption, do the former thoughts readily come again? Or have you almost to begin anew?
7. When unexpectedly addressed or asked a question which you know well enough, but have not been thinking of at the time, can you answer readily and quickly at once?
8. Which would you like better, to go on in the same familiar occupation, place, companionship, etc., or to have frequent change?

9. Do you, after leaving (for a longer time or for good) a place, room, occupation, etc., feel, as it were, homesick after it?
10. (*a*) When you have once begun something, or done it a few times (gone away, played a game), do you feel a tendency to stick to it, to do it again and again, though you no longer have any reason for it? (*b*) Or even against reason?
11. When stepping off a train before it has completely stopped, or sitting in a train when it starts or stops, do you feel a considerable shock? Or do you scarcely notice any shock at all?
12. (*a*) Do you, after a long railway journey or sea voyage, seem to hear the noise and feel the motion of the train or ship for some time? (*b*) Have you ever noticed it recurring in your dreams?

CORRECTION.

In the last issue of the Journal the reviewer of two books by Mr. Harry Price referred to him as President of the Society for Psychical Research in the year 1939, the position he actually held being that of Secretary. The President was Professor H. H. Price of Oxford University.

REVIEWS.

BRADLEY AND BERGSON: A COMPARATIVE STUDY. By Ram Murti Loomba. Upper India Publishing House, Lucknow, 1937. Pp. 187. Price Rs 2/8.

The aim of this book is to bring out the similarities between the philosophies of Bradley and Bergson, and to show them as stages in a single movement of "anti-intellectualistic idealism". It is, as explained by Professor Sen Gupta in a foreword, the first publication of research work in philosophy carried on at the Lucknow University. The book is short and the treatment not very thorough. But the points made are clearly stated (despite awkwardnesses of English grammar), and are illustrated by concise summaries of the views of the two writers. The references made to parallel issues in Indian philosophy, though again very brief, would be of interest to anyone who had knowledge of this field.

In the first chapter, Mr. Loomba draws a distinction between two ways of seeking knowledge of ultimate reality—the way of rational investigation and the way of immediate apprehension. Idealists, as well as scientific realists, have taken the former path, and thus we have today what appears as a radical opposition between the schools of Bradley and Bergson—the absolute idealist with his criterion of rational coherence, and the mystic with his criterion of intuition. But Mr. Loomba's aim is to show that these thinkers reach strikingly similar results, and this further suggests to him that idealism must ultimately end in a mystical view of the universe, that mysticism philosophically worked out must take the shape of an absolute idealism, and that Bergson represents merely a more advanced stage than Bradley of the reaction against scientific methods in philosophy.

In the next four chapters the comparison is worked out. The essential points are that for both Bradley and Bergson science cannot solve the problem of ultimate reality, that for

knowledge of this reality both rely in the end on immediate non-relational experience, that for Bradley judgment, as for Bergson intellect, is inadequate because of its relational and practical nature, and that the reality discovered, whether "the Absolute" or "Life", in each case constitutes a unity in diversity, though only in Bergson is the concrete nature of such unity made clear.

This comparison leads up to a final chapter in which the two theories are taken together as parts of a single movement of anti-intellectualistic idealism. While on the one hand, Bergson's conclusions remain essentially idealistic, on the other Bradley would be a mystic save for his scepticism as to whether the higher immediacy can be attained in our experience.

With the main points of comparison few, I think, would quarrel. In the interests of comparison, however, one meets in some places a rather forced interpretation of Bergson, as when it is said (p. 40) that Bergson's criterion of reality is really the same as Bradley's, viz., inclusiveness and internal harmony, or (p. 140) that for Bergson matter is nothing more than life as it appears to the intellect. Again, though the differences between the two views are mentioned, their importance is in some cases missed. This seems to me to apply particularly to the contrast between the timelessess of Bradley's Absolute, and the essentially temporal nature of the "élan vital". Here seems to be a very radical difference between the reality that would be discovered in Bradley's higher immediacy and the reality Bergson thinks he actually discovers by intuition. And yet on p. 146 Mr. Loomba goes so far as to say that on examination Bergson's "duration", in that it is always accumulating the past and apperceiving the future, could be considered to be virtually non-temporal.

Mr. Loomba does, of course, sympathise with that type of philosophy which seeks to discover the nature of an ultimate reality by a special philosophical method, and more especially with the "anti-intellectualistic idealism" in which he considers it to culminate. For those who do not accept this type of

philosophy, his book may not seem to raise the most important problems. It nevertheless remains an interesting and suggestive discussion of two of the most imporant of modern philosophers.

Q. B. GIBSON.

THE REDISCOVERY OF MAN. By Henry C. Link. London, Macmillan & Co., 1939. Pp. 257. Australian price: 8s. 6d.

This is not a scientific treatise but is an inspirational book—and a good one. Henry C. Link is Secretary-Treasurer of the Psychological Corporation and is Director of the Psychological Service Center in New York and has enjoyed considerable status in the field of Applied Psychology, at least since the publication of his "Employment Psychology" in 1919. He was until recently editor of the Journal of Applied Psychology. His background is sound and one needs to appreciate his purpose in the "The Rediscovery of Man" before proceeding to analyse its various statements and suggestions.

Quite early Link suggests his object. He writes (p. 89): "Millions of manuals and books testify to the tremendous codification of the habits of the mind, but there is not yet a single text-book which codifies even the a b c's of personality." "The Rediscovery of Man" is no text-book. Having written it, the author realises that. In conclusion he says, "The great task before us is first, to discover and to establish the axioms of personality. Second, to extend and to codify those axioms in terms of the personal and social concepts of living. This book represents a step in that direction."

The present title epitomises the central theme of the book when once its choice has been explained in the prefacing "Letter to a Pessimistic Friend". Link has contended before, in his "Return to Religion" (1936), and is contending again that psychology is proving, through its studies, that religion is the very foundation of personality. Psychology has "discovered" personality, its nature and, he believes, the method of its acquisition. This discovery "has led to what is

virtually a rediscovery of man himself. Man is now revalued as a creature of far greater possibilities than have usually been ascribed to him in recent years." For Link the concept of personality has lifted man out of the class of "mechanisms". Personality, he tells his "pessimistic friend", is at once a "way of life and a philosophy of living". He rejoices that for him a relict spark of the mystical has again burst into flame.

The concept of man as a cog in a big machine, as a victim of circumstance, is anathema to Link, just as it is to many another of us; but few perhaps would go with him the whole distance of claiming that "this degrading concept of man reaches its climax in the teachings of our social studies, economics, sociology, political science, anthropology" (p. 10). Psychology, apparently, has rescued man from these depths. It has found him to be capable of extraordinary powers, to be a creator capable of achieving "personality". This revolutionary outlook upon the potentialities of mankind has very great real significance. The growth of the European dictatorships is to be seen as no accident, but rather as the inevitable outcome of the concept of man as a helpless cog in a mechanical universe. The tendency has been to develop a dependent attitude of which dictatorship takes advantage. The essential condition of the success of democracy is the achievement of personality. This is the notion Link develops. And few would disagree with him thus far. It is, however, disappointing to find in Chapter III a rather weak definition of personality which, as he pursues it, leads him into a number of seeming inconsistencies and many difficulties.

The basic elements of psychological study are here said to be habits. "Psychology is essentially the scientific study of habits and their formation, first, so that those habits may be better understood, second, so that they may be better controlled" (p. 29). Every mental event is analysable into habits. The success of marriage depends upon habits acquired before marriage. People's logic depends upon their habits of thought and reasoning. Similarly personality consists of habits and skills which can be acquired. Even the I.Q. is

said to be a rough measure of the kind of habits required to get good marks in school. This seems to be stretching the notion of habit to breaking point. Link is pushing a useful idea almost to absurdity. One does not doubt the element of truth in such analyses, but undue emphasis on this one element obscures many more important things.

Consistent with this emphasis is the definition of personality offered on p. 60. Personality is "the extent to which the individual has developed habits and skills which interest and serve other people". This is a very useful definition. The whole individual is a bundle of habits. The essential differentia of personality is that it is the degree to which these habits "interest and serve other people". One bent upon inspiring people to a more "Christian" way of life can make great use of such a definition. Its emphasis is on considering other people. "Its essence is self-sacrifice, not self-gratification." It is a definition requiring unselfish service for the achievement of personality. Later on (p. 84) we are told, "Personality and its rewards are a by-product of converting one's energies into habits and skills which interest and serve other people". Personality cannot be directly acquired, it comes to those who humble themselves. The reader cannot but compare the acquisition of personality with qualification for entry into the Kingdom of Heaven. For neither can one be prepared except he forget the goal, forget self and devote himself to the service of mankind. There is little of scientific value in such a definition. Its inspirational value is probably better than its Biblical counterpart since it offers a more tangible result. But surely no scientist will accept this as a definition of personality. It seems generally agreed that personality is a product of the social order, it depends for its manifestation upon social intercourse, but it is something more besides. It takes some part of its quality from the self which it masks. Genetically the self is an integration, a product of forces within the individual. Probably in a final analysis we shall find personality is nothing more than the relation between that self and society,

dependent both upon the self and upon the group. But to dismiss it as a bundle of habits is altogether too arbitrary. Link's "personality" is rather social adjustment. One might almost say that for him personality equals "oomph". This means "personality" is not a scientific concept but a qualitative moral concept; in fact more after the nature of what psychologists have generally agreed to call "character".

Pursuing this analysis Chapter IV seeks to answer the question, "What are the habits that convert a person's energies into smooth flowing currents which interest and serve other people?" The first step in the answer is found in Link's own Personality Quotient test which requires the testee to check answers to a number of questions concerning his habits of sport, study, social activity, hobbies etc. Analysis of test responses has shown that the group of activities and habits which contribute most toward personality are physical games and competitive sports. Doing well in scholastic studies was found to have no bearing on personality, but the liking or dislike for certain subjects was claimed to be significant. Apart from the fact that a dislike or a liking for a particular subject frequently derives from a social relation with the teacher, Link presents (p. 79) a curious fact. "Arithmetic was the most significant of all the studies, especially among the boys. . . . A liking for arithmetic was found to be highly characteristic of the more effective personalities." A poor justification is offered. "Arithmetic represents a set of habits which are of basic importance in dealing with other people fairly and squarely, not only in such simple acts as sharing the cost of a luncheon, but in keeping score, in the household budget, in business, in a thousand and one social situations." Once again Link is being kind. He asks us to believe that the friend who consistently forgets to pay his share of luncheon or theatre costs merely dislikes arithmetic—I wonder. Reverting to the main emphasis of this chapter, it is this: "we are compelled to regard bodily movement as the common denominator of personality, not any bodily activity, but that carried on with or in relation to other people in play or in

work". In other words the approach to personality is through competitive games. Just why this should be so is suggested later on in chapter VIII.

Meanwhile the sermonising really begins in chapter V, "On Overcoming Fears and Worries". Here the central theme seems to be that many people are chronic worriers because their minds are too active in the wrong direction. They are failing to use their natural endowment properly. Therefore, suggests Link, since happiness is the supreme desideratum of all living they might as well be deprived of those intellectual powers. He suggests the value of the "psychic surgery" of Dr. Moniz, a Portuguese who has developed a technique for destroying the frontal lobes of the brain "with brilliant results". This area is chosen for destruction because it is "the seat of reason and imagination"—itself a questionable statement. It is reported that of twenty-one operations most of them were successful in "wholly or partly eliminating such symptoms as successive fears, worries, insomnia, delusions, crying spells, nervous indigestion, panic states and hysterical paralysis". Without questioning this report one might point out that every charlatan in psychological medicine can report cures—not because the technique has been particularly well suited but because suggestion is a force which will work through the most useless technique.

Patients who have submitted to this operation are said to experience a slowing down of the thought processes and at the same time greater physical activity. This fact Link claims to confirm advice he frequently gives that people "should use their heads less and their feet more". The implicit assumption is that there is but one energy, it can be used one way or the other but not both ways. But one frequently meets the individual who does have it both ways; and even more frequently, perhaps, one meets him who is sluggish both mentally and physically. Apart from considerations of time there is little in this advice. It is a commonplace among educators that the human organism requires wholesome exercise, and that too close confinement to mental work impairs health. In

that sense many people could use, not only their feet, but their bodies more. But to give sleep by sheer exhaustion is not to cure phobia and obsession as Link suggests on p. 96. "You have thought yourself into this fear with your mind; you can run yourself out of it with your legs." The thesis is that fears and worries are the outcome of a too active but unoccupied brain. Therefore, a patient may either be relieved of part of the brain or be given more to *do*. Or, as a third curative procedure, he might be led to think less and act more upon habits such as a moral code, a religion, a set of fixed principles. In other words happiness is to be had by faith and not by reason. So, in effect, Link advises patients to become more primitive; to avoid intellectual conflicts; to sacrifice the achievements of intellect. He recognises (p. 107) the sacrifice that psychic surgery, and even his advice, entails, but he prefers happiness to intellect. He wants simple living by faith and action and then fears and worries will have been overcome. But surely clinical psychology has taught us that readjustment on a rational level is possible and even Link would not deny that it were better to readjust than to deprive the patient of intellect.

Despite the glowing accounts in chapter V of psychic surgery, chapter VI questions the cure of alcoholism by the withdrawal of spinal fluid. Here is a malady caused by a failure of adjustment; alcoholism is a short cut to release from tension. "The essence of this concept is that the maladjusted personality is one suffering from a surplus of energies due to the lack of a proper range of habits and skills by which to express these energies smoothly" (p. 116). On this basis Link criticises the current psychiatric notion that "Most psychoneuroses are caused by fatigue". The psychiatrist says to the patient, "Rest and more rest". Link says, "Run, play, be active". Both achieve some cures, Link probably more than the psychiatrist. But it remains doubtful whether his programme can be given the general application he ascribes to it.

One would expect the orthodox psychotherapeutic method of elucidating the cause to be more effective than a programme of blind action. One agrees that there is a tendency to relieve the patient of responsibility and that tendency is bad. The orthodox psychotherapy, however, stresses the necessity for the patient's co-operation, and realises that mere analysis is not enough, but that re-education is necessary. In a sense the person must now acquire certain habits which he has failed to acquire in his youth. He must now adjust himself where formerly he has neglected to face the issue. So far Link and orthodoxy are in accord, but here they part company. Link asserts that the new habits are to be acquired blindly, by practice, while orthodoxy requires that the repressed situation be now faced and that the programme of re-education be undertaken wittingly. Both urge a maximum of action, but the one wants action for action's sake, while the other wants action along a given re-educational line.

Chapter VII presents personal security and social security as incompatible alternatives. The contention is that personal struggle, self-determination is essential to personality and that social service robs the recipients of charity of the opportunities to develop fully. Here is a curious contradiction—if some people achieve personality in Link's sense they will strive to serve other people, and yet, he says, the recipients' personalities are being undermined. The social services of a nation can hardly be regarded as forces impinging upon the formation of character. The man who has any character abhors charity, only the spineless few depend entirely upon it. But it is their lack of personality that renders them dependent, not their dependence which undermines their personalities. Link subscribes to the old belief that competition is essential to effective production. Modern psychology, to some of us, is suggesting the value and the possibility of co-operation and the use of each according to his ability. It is a fundamental fallacy for a psychologist to assume that all men have equivalent capabilities. If psychology has established anything it is the reality of

individual differences. There are some more unfortunate than others who are out of jobs and are unlikely to find them. Can we return to the barbarism of the survival of the fittest and let these poorly endowed individuals die out? Or must we organise social services? If we are persons with personalities in Link's own sense we must try to serve these unfortunate people—probably by suggesting they play competitive football on empty stomachs. Again the author seems to be in difficulties. He likes a bit of fear because it gets things done. He says (p. 145): "Social *in*security stimulates the individual to action, to struggle, and thus to progress. Social security lulls people into a state of stagnation which develops finally into a case of jitters. Social security develops in time the very fears and doubts it was intended to allay." But again he has put the cart before the horse, the cure for the attitude seems to be the achievement of better personalities.

The analysis of the psychology of sportsmanship purports to reveal the reason why competitive athletic games contribute so much to personality. In the first place, it is "because a person expends usually two or three times the energy used in solitary pursuits. . . . This lavish use of the energies tends to reduce, immediately, one of the chief causes of emotional and mental tension, of fears and worries" (p. 159). The reviewer is still sceptical enough to wonder whether the energy expended or the opportunity afforded for healthy social contact is the salutary factor. However this may be, not only psychologists but racial hygienists of all kinds will deplore with Link the increasing tendency for the majority of people to indulge in nothing more energetic than spectator-sports. The author complains that near his home is a stadium with seating accommodation for 50,000 spectators. This is bad. It represents a fantasy satisfaction. It is much like going to the Talkies or to a Play. It is more dreaming than living.

On the other hand one fails to see the necessity for *competitive* sport except in so far as it adds interest to the game, and affords further opportunities for social contacts. But Link tries to defend the competitive system—and not in

sport only—because it is in competition that the greatest degree of co-operation is achieved. In war all petty competitions are set aside and a whole nation co-operates towards one common end. But unfortunately "we have the tragic paradox that co-operation at its peak, namely in war, represents destruction at its worst" (p. 166). Surely, if co-operation can be achieved for war, it could be achieved for peace if the nation could, in peace, move towards one common end. Leadership is the need, not competition. Sport, says Link, is an ideal training ground because sportsmanship requires that the individual give himself up to uncritical action toward a common goal. The sportsman sacrifices individuality for a group ideal. This is the achievement of personality in the sense here defined. Do the Fascists of Italy and the Nazis of Germany, then, with their devotion to the State represent the summit of perfection of personality? Certainly they offer less criticism, less revolt to the Dictator-made rules. Members of Democratic nations have a difficult way of asking questions, of questioning even the rules; but does this attitude represent a lesser type of personality? Link himself, of course, wants Democratic rules, but he also wants uncritical obedience to them—a remarkable contradiction. He deplores the liberal educational policy which has been to encourage "every student to develop his own code". "This trend spells the death of co-operation, of competition, of sportsmanship, of personality" (p. 174). Link will have no liberalism in the interpretation of the rules. The rules of Democracy should not allow of interpretation by a supreme court. The rules of a religious life—the ten commandments—have by liberal interpretation ceased to function. One wonders by what means progress is to be achieved until he points the path. It is to be the path of sportsmanship—submission to the rules.

Chapters IX and X present a very mixed grill. They include brief discussions of such topics as the "gold standard", "will", "psychological tests", "mental telepathy", "Fascism", "Communism", and "Democracy".

Under the title "The Philosophy of Personality" the next chapter points out that "There is one philosophy, the very heart of which is personality, that is the philosophy represented by Christianity. The essence of Christianity is its insistence on the supreme value of the individual . . ." (p. 235). The main sentence of the chapter is to be found in italics on page 237. "The tragedy of the Christian Church is that she has tried to make a compromise with science instead of consistently denying the power of science to touch the soul of man." Doubtless scientific analysis is for the few, religious encouragement for the many, but the mental outlook of the scientist who denies the right of analysis leaves one flabbergasted. In all probability this confused mental state of the reviewer has already become apparent.

One might conclude, in the epistolary style of the author's own preface, somewhat like this—

Sydney.
30th January, 1941.

Dear Dr. Link,

Your "The Rediscovery of Man" has become one of my valued possessions. It is not because I agree with you but because I so seriously disagree. Seldom have I been stimulated to think so consistently. Never have I so filled the margins of a book with the notation of my objections.

And yet I, myself a psychologist, feel that through you I have "rediscovered man". Psychology has by its analytic attitude tended to hide man from man and most of all from the psychologist. The modern pre-occupation with "personality" should do much to reproduce the right perspective. I am very interested in your point that this "new" perspective is really that of the Bible, especially of the teaching of Jesus.

But your belief that personality is a product of faith rather than reason appals me. Personality to me is a social revelation of the self. Classical analyses of personality such as those of McDougall and Freud agree that the individual achieves a "super-ego" or as Baldwin called it, an "ethical

self"—a stage of idealised behaviour. This is the stage at which volitional action becomes possible. The will is nothing but this "self" in action. Your analysis of pp. 200 to 204 seems to me to oversimplify and to miss this valuable concept.

Out of your oversimplification arises my next objection. In your letter to Frank you complained of the erroneous "popular" notion of personality. My opinion is that you have also erred in giving the impression that "personality" is necessarily "good". You seem to use the term in much the sense in which the word "character" is used. By personality you suggest social adjustment. I should like to give the term a more general connotation. Personality is something everybody has, because it is only a relation between the individual and the social group.

To write a book in an effort to assist people to social adjustment is one thing—and a very valuable one. But to suggest that that book also offers an analysis of personality is another thing altogether. "The Rediscovery of Man" has been, and must continue to be an inspiration to preachers and teachers. Just as surely, I think, it must continue to be attacked by psychologists. The psychologist still believes that even personality is made by the function of intellect. While he realises the importance of the acceptance of ideals he does, I think, still prefer to regard the fundamental integration as the product of intellect.

Your claim to optimism disturbs me. Can it be that it is optimistic to believe that by less thinking personality is to be achieved; and through personality Democracy made safe? Is not this rather supreme pessimism? Is it not the inhibition of intellect, the acceptance of rules ready made that constitutes the psychological element of Fascism and Nazism? Individuality in its worst extreme may mean anarchy. But won't your advice, if carried to its extreme, mean apathy, which is deadly poison to democracy?

Right or wrong I believe this is a timely book. It will direct the thoughts of men to the very important subject of "man". Even those who disagree with you, perhaps they most

of all, will thank you, as I do. It seems to be part of the heritage of the psychologist to work and hope for the day when men will better understand "man". "The Rediscovery of Man" will be appreciated as a contribution to the psychological propaganda campaign. It may be that in a world gone mad with war just such a simple statement as this is needed to raise man from his squalor.

C. A. Gibb.

The 1940 Mental Measurements Yearbook. Edited by Oscar Krisen Buros. The Mental Measurements Yearbook, a Co-operative Nonprofit Service for Test Users, 32 Lincoln Avenue, Highland Park, New Jersey. 700 pages 7½ by 10½ inches. Price $6.00 (10% discount on orders sent direct to publishers).

"The Nineteen Forty Mental Measurements Yearbook" might be called the fifth publication in this excellent series produced by Professor Buros. And yet one hesitates so to designate it because in six years since the original publication in the series its growth and extension have been almost incomprehensible.

In the summer of 1935 Buros published, as a Rutgers University Bulletin, a non-critical list of tests entitled "Educational Psychological and Personality Tests of 1933 and 1934". This was a forty-four page bulletin designed to supplement "A Bibliography of Mental Tests and Rating Scales" published by Gertrude Hildreth in 1933. Five hundred copies of this bulletin were printed, and though it was widely distributed it was not reviewed.

Nevertheless in 1936 Buros issued a further bulletin of 83 pages listing again tests of 1933 and 1934 and adding tests published through 1935. This time reviews appeared, but sales still lagged.

"Educational, Psychological and Personality Tests of 1936" brought up to date the previous bulletin; and in addition it tried to provide a still greater service to test users by listing measurement books and monographs, and by publishing review excerpts of these books. Two hundred and ninety-one

books were listed and one hundred and twenty professional journals were searched for reviews. The critical portions of over six hundred book reviews were published. The new section and the detailed indexes now provided increased the bulletin to 141 pages. Favourable reviews encouraged Buros to extend still further.

The fourth publication of the series was "The 1938 Mental Measurements Yearbook" which introduced many novel features, and which had grown to a volume of 415 pages. Among the most noteworthy of these new features were: (1) Test reviews which offered critical reviews of all the tests listed. This work was done with the co-operation of one hundred and thirty-three reviewers who were all psychologists, subject matter specialists, teachers or test technicians. (2) Research and Statistical Methodology Books were listed and review excerpts published for each. In addition this volume retained the bibliography and review excerpts of Mental Measurements Books. "The Nineteen Thirty-Eight Mental Measurements Yearbook" still financed by Rutgers University, was a praiseworthy product of co-operative effort and editorial organisation.

This year "The Nineteen Forty Mental Measurements Yearbook" has come from the same co-operative effort and even greater editorial organisation and determination. An extraordinary expansion of the programme of the previous volume has taken place. And that in spite of the fact that Buros has failed to get financial assistance for the continuation and expansion of this work.

Details of the expansion are contained in the Editor's preface. In the section "Tests and Reviews": "The number of co-operating reviewers has been increased from one hundred and thirty-three to two hundred and fifty. The number of original test reviews has been increased from two hundred and thirty-one to five hundred and three. More space has been allowed to reviewers: the unsatisfactory plan of 40- to 100-word appraisals used in part in 'The 1938 Yearbook' has been abandoned for longer test reviews. . . . The representation given to other English-speaking countries has been increased

from one to sixteen reviewers—this number would have been much larger were it not for the war. . . . The presentation of titles of reviewers allows readers to understand better the viewpoints represented. . . . Old as well as new tests are reviewed in this Yearbook. . . . Finally the lists of references dealing with the construction, validation, use and limitations of specific tests have been compiled in a more thorough and comprehensive manner." In the section "Books and Reviews": More than a hundred new journals—fifty of which were in medicine—were searched for reviews. Thus this Yearbook contains review excerpts from one hundred and seventy-eight journals as compared with one hundred and forty-six represented in the 1938 volume. Further, the scope of this section has been expanded to include a number of books bordering on the field of mental measurements.

Readers familiar with the 1938 volume will notice the omission here of its section "Research and Statistical Methodology Books". The Editor promises, however, that this omitted section will be published as a separate volume early this year. Thus the 1940 Yearbook has really become two volumes.

With such an expanded coverage one would expect great changes in the format of the volume, and there is no disappointment here. The 1940 Yearbook is delightfully printed and bound. It is set in larger type on a much larger page which affords greater ease of reading. One should point out, however, that this is not a book which anyone will want to read from cover to cover. It is essentially a reference work. It is just that fact which makes two opposed viewpoints possible. One might assume that since the Yearbook is a reference book the increased size of the volume, and, in fact, its spread to two volumes are of no consequence. On the other hand one might argue that the smaller print of the 1938 Yearbook was adequate for this type of work and that it would, therefore, have been preferable to retain the small type and keep all the material between the one set of boards. Buros evidently takes the former view. The reviewer inclines

to the latter, and would have preferred the more closely set but handier volume.

On the other hand Buros announces (p. xx) his intention of providing handy and cheap information in each of the several fields covered by the "Yearbook". A new series of monographs is being planned. Each such publication will present a comprehensive bibliography of all tests in the chosen limited field (together with reviews of those tests) which have appeared in previous Yearbooks; and, in some cases, new reviews will be included. It is proposed that such separates in each field should be issued at about six-yearly intervals, more or less frequently according to the test material published. It is obvious that these monographs cannot be so up to date as the "Yearbooks", but they will afford teachers and others an opportunity to have always handy, reliable information concerning the more important tests. The first of these separates is to be devoted to tests of English and reading and will be published in late 1941 or early 1942.

While this additional service is admirable it does not compensate for the increased cumbersomeness of the "1940 Yearbook". One cannot but feel that as this expansion continues the book will be consulted less. Further, the expansion has been, in some instances, unwarranted. The more detailed bibliography given for each test is very welcome. But there must be some doubt as to the wisdom of extending test reviews, especially since reviews do occasionally overlap.

The "Books and Reviews" section of the "1940 Yearbook" occupies only about one-third of the volume. It might, however, have occupied still less. An actual case or two may make this criticism clear. "Baker and Traphagen—The Diagnosis and Treatment of Behaviour-Problem Children" was given three columns of reviews from twelve journals in the "1938 Yearbook". In the present volume it is allotted another three columns for reviews from an additional four journals. Admittedly one of these reviews sees the book from a new point of view, the others, however, do not. Again three of these additional reviews were published too late for inclusion in the 1938 Yearbook, the other was not. It is difficult to

conceive this additional three columns returning proportionate benefit to the Yearbook reader.

This is no isolated example. Similar doubt arises concerning additional space given to at least half a dozen other books.

One other facet of this same critical point concerns the large amount of space allotted to a few of the more outstanding books. Freeman's "Mental Tests" in its revised edition is represented here by eight reviews occupying six columns. More severe pruning of reviews might well have done this book justice in half the space. A similar criticism applies to the presentation of fifteen reviews of "The Psychology of Early Growth" by Gesell and Thompson; and of the four pages of reviews of McCall's "Measurement"; and of the eleven columns given to "Who Shall Survive" by Moreno; and there are many others.

To put it bluntly Buros shows signs of being carried away by his organisation. His desire to be impartial at all costs has led him to the high cost of abstracting every available review article of measurement books.

It would be misleading if this review concluded upon an even slightly disparaging note. The reviewer has no intention of creating an impression of abnormal gigantism. The "1940 Yearbook" is more valuable than its predecessors have been; and that is no mean praise. Its list of co-operating specialists is longer, it is more up to date at publication, it is more complete in coverage, it has sampled more journals for reviews.

Australian readers will be interested to realise that among new journals covered is "The Medical Journal of Australia", while THE AUSTRALASIAN JOURNAL OF PSYCHOLOGY AND PHILOSOPHY has been covered since the inception of Buros' review abstracts. Buros has, however, gone further than that. He has arranged with the Australian Council of Educational Research and with Psychologists and Educationists in this country for a more complete coverage of the Australian contribution; and "The 1942 Mental Measurements Yearbook" is to have several Australians among its ever-growing list of co-operating reviewers.

One can only wish Professor Buros and his venture every success and again offer congratulations upon the continued publication of this "Co-operative Service for Test Users".

C. A. GIBB.

PSYCHOLOGY AND PSYCHOTHERAPY. By William Brown. Fourth Edition. Edward Arnold & Co., London, 1940. Pp. 260. Price 12s. 6d.

The third edition of this book was reviewed in the Journal in December, 1934. In the present edition two appendices, on correlational psychology, have been omitted, and two chapters, "Psychological Problems of Later Life" and "Sublimation and Spirituality", added, and the revision of other chapters has been such as to bring out "the wider implications of the subject in the domains of political science and international affairs". Since from its first publication the book contained long extracts from articles contributed to various periodicals, and since it is now, with nine additional chapters, almost twice as long, a certain "patchwork" effect is not surprising; while a main trend of argument can be discerned, it cannot be said that the therapeutic problems raised have been worked out, or that the connection of much of the material with therapy has been indicated.

The author makes the usual extravagant claims of the psychologist confronted by social and political questions; we learn (p. 190) that, in the next generation, psychology "will give us command over mental nature, exceeding beyond imagination our power at the present time. It will work partly through giving us leaders in every domain—Church and State and the professions—who understand themselves *au fond*" (there being, naturally, no suggestion given as to how psychology is to get into this commanding position). Nevertheless he contends, in his final section on "The Eternal Values", that here "science needs to be supplemented by philosophy and religion. . . . Every psychologist must eventually become a philosopher, but his philosophy is not the same as his psychology. In practice he is bound to come right into metaphysics, and up against the problems of logic,

ethics and aesthetics—problems from which he cannot escape" (p. 229).

Brown, it may be said, has some sense of the fact that "cure", "normality", and the like are question-begging terms—but he has no sense of the extent to which their use is determined by the demands of dominant social groups; on the contrary, he makes the solidarist assumption throughout, and "eternal values" serve to bolster it up. Thus (pp. 188, 9): "If we analyse the patient we can give him a chance of sublimation, i.e. a direction of his primitive energies towards an end that is in harmony with his highest aspirations, and *therefore*" (my italics) "also in harmony with the highest aspirations of those around him." The use of "philosophy" to the psychologist is to enable him to make pronouncements like this. Certainly, in the chapter "Relation of Mind to Brain", Brown comes closer than elsewhere to philosophical discussion. But his arguments on the whole are weak (he says, e.g., on p. 198, that it is "an absurdity to try to explain the intellect in terms of that which needs the intellect for its explanation", though there is obviously *no* absurdity in saying that the intellect depends *for its existence* on that which depends on the intellect *for its being known*); and the only point of the chapter is to suggest a "limited" freedom of the mind in relation to the brain, so as to leave room for Brown's hortatory methods.

For, while there are a few vague references to "truth, beauty and goodness", it is along the time-worn line of altruism and self-sacrifice that people's "aspirations" are to be fulfilled. Sublimation requires the finding of objects for repressed libido, and these objects are to be found (p. 148) "in the social life of the individual, in his various communal duties, and the discharge of the claims upon him in his family and among his friends and acquaintances and in society in general." It is by his hortatory method, by his urging of such claims, that Brown is distinguished from the Freudians, but, seeing the force of much of Freud's work, he is not prepared to admit this; he considers it possible to have free

association *in the course of* "long persuasive talks with the patient" (p.100), instead of recognising that the two methods are absolutely opposed, and he is anxious to show that suggestion is present also in the Freudian procedure, viz. in the "transference" (pp. 107-110). Now no doubt the Freudian might be said to suggest to the patient that he is *able to associate,* but beyond that Brown simply misunderstands the procedure. Thus he says (p. 108), in connection with the "narcissistic neuroses", that the patient's narcissism "prevents one getting hold of him and helping him at all", whereas the point is that it prevents *him* from getting hold of "one". Similarly, in the passage on p. 148, the question is of objects being found *by,* not *for,* the repressed libido. On the same page Brown remarks that psycho-analysis brings "repressed tendencies to the surface and liberates them, and now they have to be directed along suitable channels"—a typically English middle-class conception of liberation. The same sort of contradiction comes out in Brown's discussion of the treatment of the adolescent masturbator, from whom we must (p. 127) "remove the shadow of fear and censure", though (p. 126) "it should also be made clear that the habit must stop, and stop immediately".

Throughout the book, but especially in the chapter on "Suggestion, Hypnotism, and Faith", Brown makes a particular point of *confidence* as a characteristic of the individual's well-being or "integration", not merely within himself, but with "the totality of the universe". It is here, we have seen, that religion and "eternal values" come in—the gilding of the solidarist pill. It does not take much knowledge either of Freudianism or of religious phenomena to see how closely confidence may be linked with phantasy. But it is a striking fact that Brown says practically nothing about the place of phantasy in Freudianism. His eclecticism, his contradictions and exhortations, his "not altogether" accepting the Freudian theory (p. 15), could, I suggest, be accounted for in terms of the phantasies he wishes to preserve.

JOHN ANDERSON.